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5. Persons of scientific distinction who have contributed to the advancement of Psychology shall be eligible for election as Honorary Members.

6. Honorary Members shall be entitled to all rights and privileges of membership.

7. Honorary Members may be elected from time to time on the nomination of the Council at any General Meeting of the Society.

8. The Council shall be empowered to institute Sections of the Society, each Section being concerned with a special branch or aspect of Psychology.

9. Election to a Section or Sections shall *ipso facto* constitute election to the Society as a whole and entitle those so elected to receive notice of and to take part in all General Meetings of the Society in addition to the meetings of the Section or Sections to which they belong.

10. Sections shall have the right of electing Sectional Associates who shall receive notice of and shall be permitted to attend all Meetings of the Section or Sections to which they belong. Such Sectional Associates shall however take no part in the Government of the Society or the Sections, and they shall not receive *The British Journal of Psychology.*

10 *a*. Transfer from Sectional Membership to Sectional Associateship may take place on January 1st only, and after not less than six months' notice from the Member of his wish to transfer.

11. The procedure as regards the election of Sectional Associates shall be the same as that in the case of Members.

12. Every Candidate for election to the Society shall be recommended by at least two Members who shall be prepared to furnish

information as to the Candidate's qualifications for membership. No Candidate shall be submitted for election to the Society who has not first been approved by the Council or by a Sectional Committee.

13. A Candidate who has been approved by the Council or by a Sectional Committee shall be nominated for election at the next General Meeting of the Society, or at the next Meeting of the Section respectively.

14. One full week before the Meeting at which the ballot is to take place, the Secretary shall send to each Member a balloting paper containing the names and addresses of the Candidates and the names of their proposers.

15. Any Member unable to attend a Meeting at which a ballot is to take place can vote by sending his balloting paper to the Secretary.

16. One adverse vote in five shall exclude.

17. To vote *for* a Candidate, a Member must prefix a cross to the Candidate's name; to vote *against* a Candidate, he must erase the Candidate's name. Members who wish to record a vote neither for nor against a Candidate must leave the name untouched.

18. Any Member whose subscription remains unpaid for two years may, after receiving due notice, be removed by a resolution of the Council from membership of the Society.

19. Whenever it shall be proposed to remove a Member from the Society for any reason other than failure in payment of subscription, the matter shall be brought as a formal resolution before the Council or one of the Sectional Committees (whichever it shall most concern). In the event of the resolution being passed, a ballot shall be held for the removal of the said Member at the next General Meeting of the Society, or at the next Meeting of the Section respectively. Should two-thirds of the Members voting vote in favour of the removal of the Member, that Member shall be removed from the Society.

20. If at any time it is desired to abolish any Section or Sections, to subdivide any Section or Sections, or to amalgamate two or more Sections, a Special Meeting of the Section or Sections concerned may be called for this purpose according to the conditions laid down in Rule 44. Resolutions passed at any such Meeting shall be reported by the Sectional Secretary or Secretaries concerned to the next Meeting of the Council. Should the Council approve of such resolutions, such resolutions shall immediately take effect. But should the Council formally express disapproval of any or all of the said resolutions, these resolutions shall be considered by an Extraordinary General Meeting of the Society sum-

moned in accordance with the conditions laid down in Rule 44 and the decision of this Meeting shall be final.

In the case of the desire to abolish any Section or Sections, the following alternative procedure may be followed. An Extraordinary General Meeting of the Society shall be summoned in accordance with the conditions laid down in Rule 44, and any resolution passed at this Meeting regarding the abolition of the said Section or Sections shall be final.

OFFICERS, COUNCIL AND SECTIONAL COMMITTEES

21. The business of the Society as a whole shall be conducted by a Council consisting of the President, Deputy President, Vice-Presidents, Treasurer, General Secretary, Librarian, Editors of *The British Journal of Psychology*, one representative each of every Section and Branch to be nominated by the Sectional and Branch Committees, and of six Ordinary Members, and not more than two members co-opted by the Council.

22. The President and Deputy President of the Society shall hold office for three years. The other officers of the Society shall retire annually but shall be eligible for re-election, the Deputy President shall be eligible only once for re-election, and the President shall not be eligible for re-election until after the lapse of a further period of six years. Every President shall at the termination of his period of office become a Vice-President for a period of six years. Co-opted members of the Council shall cease to hold office at the Annual General Meeting of the year of their election.

23. Every year two Ordinary Members of the Council, having served for three years, shall retire and shall be ineligible for re-election until after the lapse of one year.

24. Names of Members to serve on the Council may be suggested by the Council or by any Member by sending the proposed names to the General Secretary at least two weeks before the Annual General Meeting.

24 *a*. Nominations of representatives of the Sections and the Branches shall be sent to the General Secretary at least two weeks before the Annual General Meeting.

25. Nominations for the election of Officers shall be handed in to the General Secretary before the Meeting preceding the Annual General Meeting and be announced at the Meeting preceding the Annual General Meeting.

26. One full week before the Annual General Meeting, the General Secretary shall send to each Member a balloting list containing: (1) The names of the Council; (2) The number of the attendance of members of the Council at the Council Meetings held during the year; (3) The names of those proposed by the Council and Committees or by Ordinary Members to fill the vacancies.

27. The Society may fill all the vacancies among the officers that may occur between two Annual General Meetings by ballot, in accordance with Rule 15, at the next General Meeting, due notice having been given. Similar vacancies among the Ordinary Members of the Council shall be filled by co-option by the Council.

28. In the absence of an ex-officio Chairman (in accordance with the conditions laid down in Rule 42) the Council shall elect one of its Members to act as Chairman of Council. A quorum at a Council Meeting shall consist of five. If there be no quorum, the Members attending shall have power to transact business, subject to the approval of absent Members, to whom the resolutions adopted shall be forwarded.

29. The first Meeting of each Special Section shall be convened by the General Secretary of the Society. At this Meeting the Section shall appoint its Chairman, Secretary and Sectional Committee.

30. The Chairman and Secretary of a Section shall retire annually but shall be eligible for re-election.

31. The size of a Sectional Committee shall be determined by the Section. A quorum at a Meeting of a Sectional Committee shall be as that Committee shall decide.

32. When Sections have been formed, Rules 12, 13, 14, 15, 21, 24, 25, 26, 27, 28, 40, 41, 42, 43, 44 shall (*mutatis mutandis* and unless expressly stated to the contrary) apply respectively to the Committees, Chairmen and Secretaries appointed by these Sections.

33. The Society shall not make any dividend, gift, division or bonus in money unto or between any of its Members.

SUBSCRIPTIONS AND FINANCE

34. Each Ordinary Member shall pay an annual subscription of one guinea, which shall entitle him to receive a copy of *The British Journal of Psychology*, with the exception of such additional portions of the *Journal* as are devoted to special branches or aspects of Psychology and are published separately.

35. Ordinary Members who are members of all Sections shall pay an inclusive annual subscription of £2. 15*s*.

36. In addition to the Ordinary Membership Subscription Members shall pay an additional fee for every Section which they join. This additional fee shall entitle Members of any Section to receive such additional and separately published portions of *The British Journal of Psychology* as may be devoted to the special branch or aspect of Psychology studied by the Section. The amount of such additional fee shall be fixed by the Committee of the Section concerned in consultation with the Treasurer, and any resolution passed by the Sectional Committee with reference to such fee shall, before coming into force, receive the approval of the Council.

37. Sectional Associates shall pay a subscription the amount of which shall be determined by the Sectional Committee in consultation with the Treasurer of the Society.

38. The subscription is payable in advance and becomes due on January 1st. *The British Journal of Psychology* shall be sent only to Members whose subscriptions have been duly paid.

39. The Council shall grant to each Section permission to incur such expenses as may be necessary for the work of the Section; the approximate amount of such expenses to be determined by the Council.

MEETINGS

40. The Annual General Meeting shall be held in December, when the General Secretary's Report and the Treasurer's Statement of Accounts shall be laid before the Society.

41. The General Meetings of the Society shall be at such times and places as the Council shall decide, but in each year there shall be at least four meetings.

42. The President, Deputy President, or failing them a Vice-President, shall, whenever possible, act as Chairman at the Meetings of the Council and of the Society. In the absence of the President, Deputy President, or a Vice-President, some other Member shall, on the motion of the General Secretary or his substitute, be elected to act as Chairman.

43. The Secretary shall send to all Members of the Society notices of each Meeting at least seven days beforehand, and of the business to be transacted at the Meeting.

44. An Extraordinary General Meeting may be summoned at any time by the Council, or by the General Secretary on the written request of twenty Members; at least fourteen days' notice of such Meeting shall be sent to each Member of the Society.

VISITORS

45. Each Member shall have the privilege of introducing two visitors at any Meeting. The Council or a Sectional Committee shall however have the power of closing any Meeting or Meetings to visitors, provided notice to this effect be given on the notice convening such Meeting or Meetings. The names of visitors together with the names of Members introducing them shall be entered in a book provided for that purpose. Any Member of the Society attending a Meeting of a Section to which he does not belong must be introduced by a Member of that Section.

BRANCHES

46. The Council shall be empowered to sanction and dissolve the establishment of Branches of the Society throughout the British Empire. Members of such Branches shall pay the same subscription as that of Ordinary Members direct to the Treasurer of the Society and be entitled to the same rights and subject to the same rules as Ordinary Members. The Council shall be empowered to refund to the Treasurer of any Branch such proportion of Branch Members' subscriptions as the Council may determine to meet local expenses.

46 *a*. Any Rules made by Branches of the Society shall be first submitted to the Council for their approval.

46 *b*. The minimum number of Members necessary to form a Branch shall be 10.

46 *c*. Branches shall be required to present a report to each Annual General Meeting.

46 *d*. Branches shall have the right of electing Branch Associates under conditions similar to those governing the election of Sectional Associates.

RULES

47. The Rules of the Society may be altered at the Annual General Meeting, after notice of alteration has been given at a previous Ordinary General Meeting, or they may be altered at an Extraordinary General Meeting summoned for that purpose. Such Extraordinary General Meeting shall be held in London, and the purpose for which it is called shall be expressly stated in the notice convening the Meeting. No other business shall be transacted thereat.

BRITISH PSYCHOLOGICAL SOCIETY

LIBRARY RULES

1. The Library of the Society shall ordinarily be open for the use of members from 10 a.m. to 5 p.m. (Saturdays, 10 a.m. to 1 p.m.).

2. Persons other than members of the Society may use the Library only on obtaining permission from the Hon. Librarian. Such permission shall as a rule be granted only to scientists or research students who are known personally or by reputation to the Hon. Librarian, or who are introduced to him by a member of the Society. Such persons must in all cases have declared their willingness to conform to the rules regulating the use of the library by members.

3. Members must on each visit to the library sign the book provided for that purpose.

4. No book may be marked, annotated, mutilated, or defaced in any way.

5. A member shall be responsible to the Society for any loss or damage occurring to a book while in his use or possession.

6. No books may be removed or borrowed from the library.

LIST OF PERIODICALS AND SERIAL PUBLICATIONS CONTAINED IN THE LIBRARY (1938)

* Denotes publications from year given up to date.

Italics denote publications added to the Library since last publication of list.

**Acta Psychiatrica et Neurologica (with Supplements).* 1938–
*Acta Psychologica. 1935–
*Action et Pensée (Bulletin de la Société Internationale de Psychagogie et de Psychothérapie). 1925–
American Journal of Insanity. 1903–1915.
*American Journal of Orthopsychiatry. 1930–
*American Journal of Psychiatry. 1921–
*American Journal of Psychology. 1888–
*Annales médico-psychologiques. 1921–
*l'Année psychologique. 1909–
*Archiv fur die gesamte Psychologie. 1903–
Archiv fur Gewerbepathologie und Gewerbehygiene. 1930–1932.
Archiv für Kriminologie. 1922–1930.
*Archives de psychologie. 1902–
*Archives of Neurology and Psychiatry. 1919–
*Archives of Psychology. 1907–
*Archivo generale di neurologia e Psychiatria. 1921–
*Australasian Journal of Psychology and Philosophy. 1923–
Berichte uber den 1–9 Kongress für experimentelle Psychologie. 1904–1924.
Brain. 1914–1921.

*British Journal of Educational Psychology. 1931–
*British Journal of Medical Psychology. 1920–
*British Journal of Psychology (General Section). 1904–
*British Journal of Psychology Monograph Supplements. No. 1–
*Buletin Eugenic si Biopolitic. 1936–
Canadian Journal of Mental Hygiene. 1919–1922.
*Catholic University of America Studies in Psychology and Psychiatry. 1926–
*Character and Personality. 1932–
Columbia University Contributions to Education. (Selected numbers.)
*Comparative Psychology Monographs. 1922–
Dictionary of Psychology (*Warren*).
Die Experimentelle Pädagogik. 1905–1910.
*Educational Research Bulletin. 1928–
*l'Encéphale. 1927–
*Eugenics Review. 1922–
Fortschritte der Psychologie. 1913–1922.
Forum of Education (*now incorporated with the* British Journal of Educational Psychology). 1923–1930.
*Genetic Psychology Monographs. 1927–
Grenzfragen des Nerven- und Seelenlebens. 1900–1926.
(The) Human factor (Journal of the N.I.I.P.) (*now* Occupational Psychology). 1932–1937.
*l'Hygiène Mentale. 1927–
*Imago. 1913–
*Indian Journal of Psychology. 1926– (*set incomplete*).
*Industrial Health Research Board Reports. No. 1–
Industrial Psychology. 1926–1928.
*Industrielle Psychotechnik. 1924–
*International Journal of Psycho-Analysis. 1920–
*International Zeitschrift fur Individualpsychologie. 1924–
*Internationale Zeitschrift fur Psychoanalyse. 1913–
Journal de Neurologie et Psychiatrie. 1927–1929.
*Journal de Psychologie. 1904–
Journal fur Psychologie und Neurologie. 1902–1934.
*Journal of Abnormal and Social Psychology. 1907–
Journal of Animal Behavior. 1911–1917.
*Journal of Applied Psychology. 1917–
Journal of Comparative Neurology and Psychology. 1907–1910.
*Journal of Comparative Psychology. 1921–
*Journal of Educational Psychology. 1910–
*Journal of Educational Research. 1920–
Journal of Experimental Pedagogy. 1911–1921.
*Journal of Experimental Psychology. 1916–
*Journal of General Psychology. 1928–
*Journal of Genetic Psychology (*formerly* Pedagogical Seminary). 1891–
*Journal of Juvenile Research. 1928–
*Journal of Mental Science. 1921–

*Journal of Nervous and Mental Disease. 1926–

*Journal of Neurology and Psychiatry (*formerly* Journal of Neurology and Psychopathology). 1938–

Journal of Neurology and Psychopathology (*see* Journal of Neurology and Psychiatry). 1920–1937.

**Journal of Parapsychology.* 1937–

Journal of Philosophical Studies (*now* Philosophy). 1926–1930.

*Journal of Philosophy. 1911–

Journal of Philosophy, Psychology and Scientific Methods. 1908–1914.

*Journal of Social Psychology. 1930–

Journal of the National Institute of Industrial Psychology (*see* The Human Factor). 1924–1932.

*Journal of the Royal Anthropological Institute. 1909–

*Kwartalnik Psychologiczny. 1930–

Man. 1906–1928.

*Mind. 1915–

Nervous and Mental Disease Monograph Series. (Selected numbers.)

*Neue Psychologische Studien. (Munchen.) 1923–

*Occupational Psychology (*formerly* The Human Factor). 1937–

*Organon. 1936–

*Pedagogical Seminary and Journal of Genetic Psychology (*now known as* Journal of Genetic Psychology). 1891–

*Personnel Journal. 1927–

*Philosophical Review. 1915–

Philosophische Studien (Wundt). 1883–1902.

*Philosophy (*formerly* Journal of Philosophical Studies). 1931–

Praktische Psychologie. 1919–1922.

Proceedings of the Aristotelian Society. 1916–1923.

*Proceedings of the National Academy of Sciences. 1929–

*Proceedings of the Society for Psychical Research. 1910–

*Psyche. 1921–

*Psychoanalytic Quarterly. 1932–

*Psychoanalytic Review. 1913–

die Psychoanalytische Bewegung. 1929–1933.

*Psychological Abstracts. 1927–

*Psychological Bulletin. 1904–

Psychological Clinic. 1908–1935.

Psychological Register. 1929, 1932.

*Psychological Review. 1894–

Psychological Review Index (*now incorporated in* Psychological Abstracts). 1894–1935.

*Psychological Review Monographs. No. 1–

Psychologische Arbeiten (ed. Kräpelin). 1895–1928.

*Psychologische Forschung. 1922–

Psychologische Studien (Wundt) (*see* Neue Psychologische Studien). 1906–1917.

*Psychometrika. 1936–

**Revista de Psihologie.* 1938–

*Revue Française de Psychanalyse. 1927–
*Rivista di Neurologia. 1928–
*Rivista di Psicologia. 1905–
Science Progress. 1917–1933.
*Scientia. 1926–
Training School Bulletin. 1929–1931.
le Travail Humain. 1933–
*University of California Publications. 1916–
Yale Psychological Laboratory Studies. 1892–1902.
*Zeitschrift fur angewandte Psychologie. 1908–
*Zeitschrift fur angewandte Psychologie. Beihefte.
Zeitschrift fur Neurologie und Psychiatrie. 1910–1914.
*Zeitschrift fur pädagogische Psychologie. 1899–
Zeitschrift fur psychoanalytische Pädagogik. 1926–1937.
*Zeitschrift fur Psychologie. 1890–
Zeitschrift für Psychotherapie und medizinische Psychologie. 1909–1915.
*Zeitschrift für Sinnesphysiologie. 1908–

In addition to the above, the Library contains a few numbers only of the following publications:

Arbeiten aus dem Psychologischen Institut der Universität Munchen.
Archivos Brasileiros de Hygiene Mental.
Bulletin de l'Institut Général Psychologique.
Journal of the Department of Letters, University of Calcutta.
Mental Measurement Monographs.
Pädagogisch-psychologische Arbeiten (aus dem Institute des Leipziger Lehrervereins).
Scandinavian Scientific Review.
Smithsonian Institution Publications.
Tohoku Psychologica Folia.
Travaux Psychologiques de l'Université Jean Casimir de Lwów, Poland.

VOLUME XXX JULY, 1939 PART 1

INTELLIGENCE TEST SCORES OF MENTALLY DEFECTIVE PATIENTS AND THEIR RELATIVES

BY L. S. PENROSE

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I. *Introduction* (pp. 1–2).
II. *The method of determining mental ratio* (pp. 2–9).
III. *Analysis of Stanford-Binet mental ratios* (pp. 9–15).
IV. *Analysis of Porteus Maze test results* (pp. 15–17).
V. *Sex differences* (p. 17).
VI. *Conclusion* (pp. 17–18).
References (p. 18).

I. INTRODUCTION

IN a recently published survey of the family histories of 1280 cases of mental defect (1), the approximate mental capacity of each relative was recorded. Not every relative could be given an intelligence test, and so the ratings of mental capacity which were made in that study had to be based upon a standard obtained from the actual results of testing a large sample. The tests used on this sample population of mentally defective patients and their relatives were the Stanford Revision of the Binet Scale and the Vineland Revision of the Porteus Maze Series. In the present communication, the results of these tests on the sample population are analysed.

The range of chronological age of persons tested was so wide that a special enquiry had to be undertaken to ascertain how the score of a given person was likely to change as he grew older. In the previously published survey only a crude conventional estimate of the intelligence quotient was made. This estimate, though sufficiently accurate to distinguish between normality, mental dullness and mental defect, was not fine enough to enable arithmetical analysis to be undertaken to obtain mean values of the intelligence measurements. The estimation of arithmetic mean values was the intention of the work recorded in the present paper. The method of assigning to each person tested a mental ratio independent of chronological age is described here in § II. A Binet mental ratio thus calculated was assigned to 682 relatives of 291 patients and a Porteus mental ratio to 481 relatives of 211 patients.[1]

The relatives of patients were first ascertained by Miss D. A. Newlyn in the great majority of instances. The testing was carried out by Miss M. V. Matthews and Mr J. C. Raven. Most of the available relatives were children of school age, and thanks are due to the many education authorities who kindly gave facilities for the testing of children in schools. In the preparation of the data for publication, I have had the most valuable assistance of Mr and Mrs J. C. Raven and of Miss H. Lang Brown.

II. THE METHOD OF DETERMINING MENTAL RATIO

In order to be in a position to compare the intelligence of a child with that of an adult, a mental measurement had to be devised which was independent of age. Sometimes the patient was elderly and the relative young; in other instances, the patient was of school age and the relative adult. The Stanford-Binet scale measures mental age with fair accuracy during the years of chronological age from 8 to 12, but it is not standardized for adults. After about the age of 12 or 13, and probably earlier, the average Binet score does not increase at a rate comparable to the increase in chronological age, partly because the individual intelligence ceases to grow so fast and partly because the scale of tests comes to an end. Burt's average figures for performance of children, even on his revised scale, show that normal children of 14½ years of age score only 13 years and 8 months (2). It is sometimes tacitly assumed that the intelligence grows in linear fashion with chronological age up to a point between 14 and 16 years, and that it then suddenly stops growing. Whether or not this is so, mental ability as measured by the Binet I.Q. certainly declines. There are no exact published figures to show where the maximum Binet mental age lies or how it is reached. The writer has therefore made an attempt to measure approximately the growth of ability on the Binet test from early years up till adult life.

Material available for the investigation of the ability to score on the Binet tests at different ages was of two kinds, namely, the single test scores of 644 school children and the records of defective patients who had been retested at intervals of several years. The 644 school children were relatives of patients.[2] Their mean Binet scores were below the general average; the mean intelligence quotient tended to decline with age in a manner similar to that observed in data collected by other investigators (Burt (2), Herrmann & Hogben (3)). The age range of the tests was from 4½ to 14½ years (see Table I). The second kind of material consisted of over 1500 pairs of test scores—each pair of scores from one patient in the Royal Eastern Counties' Institution. The interval between the tests varied from 2 to 11 years but was usually 4 or 5 years. The age of the patient at the first test varied from 6 to 31 years. Inspection of these figures clearly showed that, although the I.Q. tended to decline as the patient's chronological age increased, the mental age itself actually continued to increase during almost the whole period, though the increase after the age of 20 was slight. The growth of ability on Binet tests was, in this group of defective patients, not unlike the growth of physical stature. It increased rapidly between the ages of 6 and 15, but the rate became gradually less and the maximum was reached after the age of 20. Insufficient data were available to discover in detail what changes took place after the age of 30 years, but it appeared probable that a decline set in which was at first only slight and later became quite marked.

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[1] The detailed results of these tests cannot be included here, but may be obtained on application to Dr L. S. Penrose, Research Department, Royal Eastern Counties' Institution, Colchester.

[2] There were thirty-eight adult relatives tested.

Table I. *Stanford-Binet test scores of* 644 *school children* (*relatives of patients*)

Mean chronological age	No. of children	Mean score	Intelligence quotient
5·0	25	4·61	92·2
6·0	55	5·61	93·5
7·0	58	6·40	91·5
8·0	78	7·30	91·2
9·0	77	8·01	89·0
10 0	80	8·90	89·0
11·0	63	9·72	88 4
12·0	65	9·62	80·2
13·0	85	10·95	84·2
14·0	58	11·17	79·8

In order to use the material to measure the growth of Binet ability, the assumption was made that the curve of growth for each degree of intelligence was similar to every other; that is to say, the proportional yearly increase (or decrease) was assumed to be independent of grade.[1] It is doubtful whether this assumption is correct where very high or very low grades of intelligence are concerned, but it is probably fairly true for the great majority of intelligence scores recorded in the data. In the present data, the proportional yearly increment in score for a given age was measured by combining pairs of tests in the following way. For each patient there were two scores, y_1 and y_2, which had been obtained at ages x_1 and x_2[1], respectively. For the age x_1, the value $\frac{y_2-y_1}{y_1\,(x_2-x_1)}$ was termed 'the subsequent proportional yearly increment'. Similarly, for the age x_2, $\frac{y_2-y_1}{y_2\,(x_2-x_1)}$ would be 'the previous proportional yearly increment'. For each chronological age x, the proportional increments were collected together and mean values obtained, i.e.

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[1] According to Richardson (4), the S.D. of intelligence is proportional to score plus a constant.

$$\frac{\Sigma\,(y_2-y_1)/y}{n} \quad \text{and} \quad \frac{\Sigma\,(x_2-x_1)}{n},$$

where n was the number of pairs. At 16 years, for example, there were 159 pairs of tests available for calculating the subsequent proportional yearly increment and 144 pairs for calculating the previous increment: that is, 159 patients of mean age 16 years were retested subsequently and 144 patients of the same mean age had been previously tested. The mean subsequent proportional increment was found to be 0·037 during an average time interval of 5·88 years. The corresponding figures for the previous increment were 0·134 during a period of 3·97 years. From such figures as these, the gradient of the proportional growth at any given age could be estimated.

If s represents the subsequent proportional increment in a period of time x_s and p the previous increase in the period of time x'_p, the mean increment per year, at the point from which all the intervals are measured, is

$$\frac{(s\,.\,x_p)/x_s+(p\,.\,x_s)/x_p}{x_p+x_s}$$

(if the squares of s and p are neglected).[2] Thus, the gradient of the curve at 16 years (R_{16}) is

$$\left(\frac{0{\cdot}037\times 3{\cdot}97}{5{\cdot}88}+\frac{0{\cdot}134\times 5{\cdot}88}{3{\cdot}97}\right)\left(\frac{1}{5{\cdot}88+3{\cdot}97}\right),$$

and this expression has the value of 0·023.

Similar values were calculated for other points on the scale, namely, at each year of chronological age from 12 to 31.

Between the ages of 6 and 11, it was not possible to obtain enough pairs to estimate accurately the previous increment. The pairs of tests which represented subsequent intervals were, therefore, divided into two portions as nearly equal in number as possible, and two estimates of the subsequent increments at different time intervals were made available. For example, at the year 9 there were 34 pairs with intervals 9–11, 9–12 or 9–13 between test and retest, and 42 pairs with intervals of 9–15 or a greater period of years. For the shorter period, whose mean interval was 3·85 years, s had the value 0·260, and, for the longer period (mean interval 7·95 years), the value of s was 0·357. From two readings on the same side of a point the estimation of the gradient at that point is not so accurate as when the readings are on opposite sides of the point; the formula is the same as that for readings taken on either side but the items are subtracted instead of added. Thus, the gradient R_9 has the approximate value

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[1] x_1 was always less than x_2.

[2] The formula is based on the assumption that the growth curve for any short distance has the properties of a circle.

$$\left(\frac{0{\cdot}260\times 7{\cdot}95}{3{\cdot}85}-\frac{0{\cdot}357\times 3{\cdot}85}{7{\cdot}95}\right)\left(\frac{1}{7{\cdot}95-3{\cdot}85}\right),$$

or 0·089.

The values R_6, R_7 to R_{31} are measures of the gradient on a curve whose relation to the curve of growth of absolute score is that $\frac{(dy/dx)}{y}=R$, where y is the absolute score and x the age of the subject (see Table II). To obtain a curve showing the growth of the absolute score, observed values of R must be integrated and antilogs taken in accordance with the formula

$$\int R_{dx}=\int\frac{1}{y}\,dy=\log_e yk,$$

where k is a constant. Integration was performed by adding $\frac{1}{2}R_6$, $\frac{1}{2}R_6+\frac{1}{2}R_7$, $\frac{1}{2}R_7+\frac{1}{2}R_8$, etc., cumulatively. The natural antilog of the integral values was then obtained. It then only remained to fix, for general purposes, the constant k, which solely determines whether the curve is high or low and which, therefore, is fixed by the average level of mental ability.

From evidence collected by other workers, it appeared that the Binet test gives most reliable and accurate results in the period between 10 and 11 years of chronological age. It was therefore assumed that, for normal individuals, the score at 10·5 years should be fixed at 10·5. After making this adjustment, the curve of growth of intelligence could be drawn. It is admittedly an estimate for normals based on defectives but, since the use to which it is put in the present communication is mainly to deal with people of intelligence somewhat below the normal, this is not a serious fault.

Table II. *Calculation of curve of growth of Binet ability*

Chronological age in years	No. of pairs of tests			$\frac{(dy/dx)}{y}=R$	Cumulant (c) $\Sigma R\,\Delta x$	$e^c=yk$	y
	Previous interval	Subsequent intervals					
		(i)	(ii)				
6·0	—	4	2	+0·12016	0·06008	1 062	6·79
7·0	—	10	7	+0·12828	0·18430	1·202	7·68
8·0	—	23	16	+0·08948	0·29318	1·341	8·57
9·0	—	34	42	+0·08871	0·38228	1·466	9·37
10 0	—	24	28	+0·07559	0·46443	1·591	10·17
11·0	—	37	40	+0·04966	0·52705	1·693	10·82
12·0	16	47	—	+0·05259	0·57818	1·782	11·39
13·0	92	138	—	+0·03815	0·62355	1·865	11·92
14·0	50	66	—	+0 03922	0·66223	1·949	12·45
15·0	77	101	—	+0·02348	0·69358	2·001	12·79
16·0	144	159	—	+0·02267	0·71666	2·046	13·07
17·0	59	113	—	+0 01903	0·73751	2·090	13·35
18·0	41	72	—	+0·01755	0·75580	2·128	13·60
19·0	43	62	—	+0·00908	0·76911	2·158	13·79
20·0	95	78	—	+0·00544	0·77637	2·174	13·89
21·0	148	90	—	+0·00941	0·78380	2·190	13·99
22·0	80	58	—	+0·00559	0·79130	2·207	14·10
23·0	64	60	—	+0·00766	0·79792	2·221	14·19
24·0	72	50	—	+0·00535	0·80443	2·236	14·29
25·0	98	59	—	+0·00261	0·80841	2·245	14·35
26·0	77	50	—	+0·00876	0·81409	2·258	14·43
27·0	69	41	—	+0·00661	0·82178	2·275	14·54
28·0	68	27	—	+0·00399	0·82708	2·287	14·61
29·0	68	26	—	+0·00248	0·83031	2·294	14·66
30·0	56	20	—	−0·00043	0·83133	2·295	14·66
31·0	48	22	—	+0·00446	0·83335	2·301	14·70

To obtain a complete idea of how scores on Binet tests behave over the whole range from 4 to 30 years of chronological age, a composite curve had to be constructed from the two types of data. The tests of the school children reach from 4 to 14 years and the accurate part of the calculated curve reaches from 10 years onward. By assuming, in both cases, that the Binet test gives correct results at 10·5 years, the following comparable scales were obtained: the correspondence between the two scales was fairly good for the years 10–14 where they overlap. The figures, in Table III, for the school children were obtained by multiplying the mean score given in Table I by a factor, 1·125, in order to bring them up to the normal grade at 10·5 years. In order to make use of these figures for the purpose of calculating approximate mental ratios, a smooth curve had to be obtained which would give the supposed true values of normal mean Binet scores at different ages. Between the ages of 5 and 10 for the school children, a hyperbola was fitted. The values obtained from the continuation of this curve below the age of 6 are given in brackets at the top of the right-hand column in Table III. From 10 to 31 years, the

Table III. *Estimates for normal mean scores on Binet test at different chronological ages*

Chronological age in years	Mean scores of school children × 1·125	Calculated values based on pairs of test scores of patients	Composite curve of Y (estimated true values)
3	—	—	(3·32)
4	—	—	(4·29)
5	5·18	—	(5·26)
6	6·31	(6·79)	6·25
7	7·20	(7·68)	7·19
8	8·21	(8·57)	8·13
9	9·01	(9·37)	9·09
10	10·01	10·17	10·00
11	10·94	10·82	10·82
12	10·83	11·39	11·53
13	12·32	11·92	12·12
14	12·57	12·45	12·58
15	—	12·79	12·94
16	—	13·07	13·21
17	—	13·35	13·44
18	—	13·60	13·62
19	—	13·79	13·77
20	—	13·89	13·89
21	—	13·99	13·99
22	—	14·10	14·08
23	—	14·19	14·18
24	—	14·29	14·29
25	—	14·35	14·37
26	—	14·43	14·45
27	—	14·54	14·51
28	—	14·61	14·58
29	—	14·66	14·64
30	—	14·66	14·71
31	—	14·70	—
35	—	—	(14·41)
40	—	—	(14·01)
45	—	—	(13·50)
50	—	—	(13·00)
55	—	—	(12·52)
60	—	—	(12·06)

curve was smoothed out by a difference method so that the steps from year to year diminished regularly. Beyond the age of 30, it is probable that Binet scores decline but the degree of decline could not be accurately estimated from the tests and retests of patients. An estimate was, however, necessary for a few elderly patients and figures were chosen which agreed both with the small amount of data derived from tests of older patients and the curves given in a paper by C. C. & W. R. Miles (5) on the relation of intelligence scores to chronological age from early to late maturity. The continued rise in Binet score, up to 30 years, in the present estimate is comparable to the results obtained by Miles & Miles for tests of verbal experience or arithmetical ability.

For general use in calculating mental ratios (M.R.), the values in the right-hand column of Table III are changed into their corresponding reciprocals and the inverted values are multiplied by 100. To obtain the M.R. independently of age, the Binet score is multiplied by the appropriate M.R. factor as given in Table IV. The intermediate values of $100/Y$ have to be determined by the method of differences. Thus, suppose a child aged 6 years 9 months to have a Binet score of 5 years 2 months: to obtain the M.R. we find $100/Y$, which is between 14·9 and 13·9, i.e. 14·4 and 14·4 is then multiplied by $5\frac{2}{12}$ or 5·17. The result is an M.R. of 74.

Table IV

Chronological age in years	Mental ratio factor* $100/Y$	Chronological age in years	Mental ratio factor* $100/Y$
3·0	30·1	16·0	7·57
3·5	26·2	16·5	7·50
4·0	23·3	17·0	7·44
4·5	20·9	17·5	7·39
5·0	19·0	18·0	7·34
5·5	17·4	18·5	7·29
6·0	16·0	19·0	7·26
6·5	14·9	19·5	7·23
7·0	13·9	20·0	7·20
7·5	13·1	21·0	7·15
8·0	12·3	22·0	7·10
8·5	11·6	23·0	7·05
9·0	11·0	24·0	7·00
9·5	10·5	25·0	6·96
10·0	10·0	26·0	6·92
10·5	9·59	27·0	6·89
11·0	9·24	28·0	6·86
11·5	8·94	29·0	6·83
12·0	8·67	30·0	6·80
12·5	8·45	35·0	6·94
13·0	8·25	40·0	7·14
13·5	8·10	45·0	7·41
14·0	7·95	50·0	7·69
14·5	7·83	55·0	8·05
15·0	7·73	60·0	8·29
15·5	7·65		

* To obtain the M.R., multiply the individual's Stanford-Binet score by the factor appropriate to his chronological age.

The Stanford-Binet test gives a good estimate of general intelligence though it gives verbal ability undue weight. For comparison with results obtained by observing the correspondence of Binet ability in patient and relative, a second set of data was obtained in which the subjects were given the Porteus Maze Test (Vineland Revision). An analysis of the growth of Porteus Maze ability was not attempted, but the mean Binet and Porteus scores for 956 individuals, of all ages and grades who were given both tests, were put side by side. The mean score on the Porteus test follows the mean Binet score very closely (Table V) until after the mental age of 10 years; above this mental age the Porteus score is relatively lower. A small correction is therefore necessary for Porteus scores above 10 years if they are to be treated as equivalent to Binet scores for purposes of calculating mental ratio. The correction actually applied in the present study is given in Table V.

Table V

No. of individuals	Mean Binet score in years	Mean Porteus score in years	Correction factor to be multiplied by Porteus score
10	3·0	3·50	—
23	4·0	3·89	—
44	5·0	5·43	—
100	6·0	6·06	—
142	7·0	7·00	—
152	8·0	8·15	—
146	9·0	9·03	—
127	10·0	10·15	1·00
72	11·0	11·09	1·01
67	12·0	11·56	1·03
43	13·0	12·36	1·05
19	14·0	13·00	1·08
11	15·0	13·41	1·12

III. ANALYSIS OF STANFORD-BINET MENTAL RATIOS

In a previous publication (1) attention has been drawn to the finding that, when propositi are separated into groups of comparable mental grade, defect is seen to be more common among relatives of simpletons than among relatives of idiots. This inverse regression is clearly shown in the data which are discussed here. Table VI gives the mean M.R. of sibs of patients grouped by similar mental ratio. Patients for whom M.R. ranges from 0 to 29, i.e. idiots and low-grade imbeciles, have sibs whose mean M.R. is about 90. In the lower part of the table the mean M.R. of relatives is seen to be only about 82, and it can be even less than

Table VI. *Stanford-Binet tests: mean mental ratios of sibs compared with mean mental ratios of patients*

No. of pairs	Mental ratio of patient		Mental ratio of relative	
	Range	Mean	Mean	S.D.
21	0–9	5·3	87·5	17·3
31	10–19	16·7	90·0	22·2
28	20–29	24·7	91·3	9·3
21	30–39	34·3	80·1	27·2
19	40–49	44·9	85·4	17·2
31	50–59	54·1	90·1	20·0
28	60–69	63·8	83·6	16·1
33	70–79	72·6	81·5	10·1
6	80–89	85·1	82·2	8·0
3	90–99	91·0	84·3	12·7
221	Total	43·1	86·3	18·1

that of the patients themselves. In Table VII, where relatives other than sibs are included, the same peculiarity is noticeable. One psychopathic patient with a Binet score above the normal average had a child who was feeble-minded: this exceptional case might perhaps have been excluded from the survey.

Table VII. *Stanford-Binet tests: mean mental ratios of relatives (any degree) compared with mean mental ratios of patients*

No. of pairs	Mental ratio of patient		Mental ratio of relative	
	Range	Mean	Mean	S.D.
24	0–9	5·5	88·9	17·0
44	10–19	16·2	91·7	21·1
64	20–29	24·2	94·5	16·7
45	30–39	35·3	83·5	21·9
116	40–49	46·9	84·9	17·4
162	50–59	54·5	86·9	16·3
119	60–69	63·5	87·5	16·9
72	70–79	73·6	81·7	15 4
29	80–89	85·1	85·1	13·8
6	90–99	91·3	82·0	10·1
0	100–109	—	—	—
1	110–119	115·0	65·0	0·0
682	Total	50·2	86·8	17·7

The other phenomenon besides the inverse regression which is characteristic of data of the kind dealt with here is shown by the fact that, though idiots are less likely than mild cases to have defective relatives, they are more likely to have relatives who are idiots. This peculiarity is expressed in Table VI, and especially in Table VII, by the decrease in the standard deviation of the scores of relatives with the rise of the mean score of the patients. Thus, the standard deviation of the scores of relatives of idiots and imbeciles, in Table VII, is seen to be between 17 and 20, whereas the standard deviation of scores of relatives of patients with M.R. 70–99 is between 10 and 15. The nature of the distribution can be easily appreciated by referring to the scatter diagrams (Tables VIII and IX). The distribution of M.R. in relatives of those patients whose own ratios' range of M.R. is from 2 to 37 shows a tendency to bimodality. The scores of the relatives cluster around a horizontal line drawn between 92 and 97, but there are one or two scores far below this figure. The number of very low scores, although small, is greater, in proportion to the number of normals, in this group than in the group of relatives of patients whose own range of M.R. is 39 or over. The distribution of M.R. in the tables can be attributed to a tendency for the relative to resemble the patient accompanied by a tendency for instances of such resemblance to be rarer when the patient is an idiot than when he is a

Table VIII. *Stanford-Binet test: mental ratio of sib compared with mental ratio of patient*

Mental ratio of sib	Mental ratio of patient 2	7	12	17	22	27	32	39	42	47	52	57	62	67	72	77	82	87	92	Total
142	.	.	.	.	.	.	.	.	.	.	1	.	.	.	.	.	.	.	.	1
137	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	0
132	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	0
127	.	.	.	.	.	.	.	.	.	.	1	.	.	.	.	.	.	.	.	1
122	.	.	.	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1
117	.	.	.	3	.	.	.	.	.	1	.	.	.	.	.	.	.	.	.	4
112	.	.	.	2	1	1	1	.	.	.	1	.	.	.	.	.	.	.	.	6
107	.	1	1	4	.	.	.	2	.	.	.	1	.	.	.	.	.	.	.	9
102	2	2	1	.	1	1	1	.	1	2	3	1	.	.	.	.	.	.	.	15
97	.	3	.	2	1	6	1	.	1	.	5	2	5	4	2	3	.	1	1	37
92	2	1	.	3	2	1	2	2	2	2	2	3	.	3	4	1	.	.	1	31
87	1	2	.	3	5	3	1	4	2	.	2	.	3	3	2	1	.	1	.	33
82	1	1	.	.	.	2	.	.	1	1	1	1	4	.	3	1	.	2	.	18
77	.	3	.	4	.	4	2	1	1	2	.	.	1	1	5	.	.	1	.	25
72	1	.	1	2	.	.	.	1	.	1	.	.	.	.	8	1	.	.	.	15
67	.	.	.	1	.	.	.	.	.	.	.	3	.	.	.	.	1	.	1	6
62	.	.	1	.	.	.	.	.	.	.	1	1	1	.	1	.	.	.	.	5
57	.	.	.	1	.	.	.	.	.	1	.	.	1	.	.	1	.	.	.	4
52	.	.	.	.	.	.	.	.	.	.	.	1	.	.	.	.	.	.	.	1
47	.	.	.	.	.	.	1	.	.	.	1	.	1	.	.	.	.	.	.	3
42	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	0
37	.	.	.	.	.	.	.	.	.	1	.	.	.	.	.	.	.	.	.	1
32	.	.	.	.	.	.	.	.	.	.	.	.	.	1	.	.	.	.	.	1
27	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	0
22	.	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1
17	.	.	.	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1
12	.	.	.	.	.	.	.	1	.	.	.	.	.	.	.	.	.	.	.	1
7	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	0
2	.	.	.	.	.	.	.	1	.	.	.	.	.	.	.	.	.	.	.	1
Total	7	14	4	27	10	18	9	12	8	11	18	13	16	12	25	8	1	5	3	221

Table IX. *Stanford-Binet test: mental ratio of relative (any degree) compared with mental ratio of patient*

Mental ratio of relative	Mental ratio of patient																								Total
	2	7	12	17	22	27	32	37	42	47	52	57	62	67	72	77	82	87	92	97	102	107	112	117	
142	.	.	.	.	1	.	.	.	.	.	1	.	.	.	.	.	.	.	.	.	.	.	.	.	2
137	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	0
132	.	.	.	.	1	.	.	.	.	.	.	.	1	.	.	.	.	.	.	.	.	.	.	.	2
127	.	.	.	.	.	.	.	.	.	.	1	.	.	.	.	.	.	.	.	.	.	.	.	.	1
122	.	.	.	2	.	.	.	.	.	.	1	.	.	1	.	.	.	.	.	.	.	.	.	.	4
117	.	.	.	3	1	.	.	1	.	3	.	1	.	3	.	.	.	.	.	.	.	.	.	.	12
112	.	1	1	4	3	2	1	.	.	3	3	4	2	1	1	.	.	.	.	.	.	.	.	.	26
107	.	1	1	4	5	.	.	3	1	4	1	4	2	1	1	2	1	.	.	.	.	.	.	.	31
102	2	2	2	.	3	4	1	1	3	3	9	9	8	3	1	1	.	1	.	.	.	.	.	.	53
97	.	3	1	4	4	8	3	1	5	11	9	6	15	8	3	5	1	6	1	.	.	.	.	.	94
92	3	1	.	4	6	5	2	7	3	15	11	14	11	5	8	1	3	.	1	.	.	.	.	.	100
87	1	3	.	4	7	5	3	8	5	8	13	16	12	9	4	3	4	3	1	.	.	.	.	.	109
82	1	1	.	1	1	2	.	1	3	16	8	8	6	2	5	9	.	3	.	.	.	.	.	.	67
77	.	3	.	4	.	4	2	4	3	8	4	5	5	5	6	3	.	1	1	.	.	.	.	.	58
72	1		1	3	.	.	.	1	.	6	3	5	3	1	9	2	.	2	1	.	.	.	.	.	38
67	.	.	.	1	.	.	1	.	1	3	3	7	3	1	1	.	1	.	1	.	.	.	.	1	24
62	.	.	1	.	.	.	.	1	1	.	4	4	1	1	2	.	1	.	.	.	.	.	.	.	16
57	.	.	.	2	1	.	.	.	.	3	.	2	1	2	.	2	.	.	.	.	.	.	.	.	13
52	.	.	.	.	.	.	.	.	.	3	3	1	1	.	.	.	.	2	.	.	.	.	.	.	10
47	.	.	.	.	.	.	1	.	.	1	1	.	1	.	1	.	.	.	.	.	.	.	.	.	5
42	.	.	.	.	.	.	.	1	.	2	.	.	2	.	.	.	.	.	.	.	.	.	.	.	5
37	.	.	.	.	.	.	.	.	.	1	.	.	.	.	1	.	.	.	.	.	.	.	.	.	2
32	.	.	.	.	.	.	.	.	.	.	.	.	.	1	.	.	.	.	.	.	.	.	.	.	1
27	.	.	.	.	.	.	.	.	.	.	.	.	1	.	.	.	.	.	.	.	.	.	.	.	1
22	.	1	.	.	.	.	.	.	.	.	.	1	.	.	.	.	.	.	.	.	.	.	.	.	2
17	.	.	.	1	.	.	.	.	.	1	.	.	.	.	.	1	.	.	.	.	.	.	.	.	3
12	.	.	.	.	1	.	.	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	2
7	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	0
2	.	.	.	.	.	.	.	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1
Total	8	16	7	37	34	30	14	31	25	91	75	87	75	44	43	29	11	18	6	0	0	0	0	1	682

simpleton. If these tendencies were regular the entries on Table IX should lie either along the horizontal line or the diagonal line indicated. As it is, however, the entries merely cluster around these two lines. The multitude of genetic and environmental factors which determine the M.R. of a given person blur the effect of segregation, especially when the two lines are close together.

The dual tendency of the scattering of mental ratios of relatives renders comparison of intelligence of relatives of different degrees difficult unless the grade of patient is specified. To facilitate such comparisons the patients were divided into two groups. In one group the M.R. was below 50 and in the other it was 50 or above; the reasons for subdivision at this point will be discussed later. The results are given in Table X. Patients with M.R. below 50 had relatives whose mean M.R. was 88·2 and those with M.R. above 50 had relatives with mean M.R. 85·8. The regression towards the normal is therefore much more marked in the case of the relatives of the first group of patients than in the case of the relatives of the second. If the patients' sibs (Table X) are first examined from this point of view, it is seen that, for those whose M.R. is 24·2, the sibs have a mean M.R. of 87·4: this mean has regressed at least four-fifths of the distance towards an M.R. 100 which might be presumed to be the norm for the general population in urban areas. The patients whose

Table X. *Stanford-Binet tests: mean mental ratios*

The patients are divided into groups according to mental ratio above or below 50; the relatives are classified according to degree of relationship.

		Mental ratio of patient			Mental ratio of relative	
Type of relationship	No. of pairs	Range	Mean	S.D.	Mean	S.D.
Female patient's child	14	0–49	44·7	3·4	76·2	14·1
	77	50 +	64·8	11·9	77·5	17·3
	91	Total	61·7	13·2	77·3	16·9
Patient's sib	120	0–49	24·2	12·8	87·4	19·5
	101	50 +	65 8	10·6	84·9	15·5
	221	Total	43·1	23·9	86·3	18·1
Patient's half-sib	16	0–49	30·3	11·4	93·8	17·3
	37	50 +	65·1	12·0	85·6	17·9
	53	Total	54·6	19·9	88·1	18·0
Patient's nephew or niece	74	0–49	33·9	11·5	95·4	16·9
	106	50 +	62·5	9·3	90·8	13·6
	180	Total	50·7	12·5	92·7	15·1
Patient's first cousin or equivalent relationship	69	0–49	42·3	11·6	82·8	17·2
	68	50 +	61·0	9·2	89·2	15·2
	137	Total	51·6	14·0	86·0	16·4
All types	293	0–49	32·2	14 0	88·2	18 7
	389	50 +	63·8	10·8	85·8	16·5
	682	Total	50·2	19·8	86·8	17·7

mean M.R. is 65·8, however, have sibs whose mean M.R. is 84·9. If mild mental impairment were inherited by multiple Mendelian additive factors, the sibs should regress towards the norm for the general population. On this hypothesis, the norm of M.R. for the general population from which these families have been drawn is 104·0.

If the group of patients' children is examined, the mean M.R. is seen to be almost the same whether the patients' own M.R. is below 50 or above. The tendency of the children to regress towards the normal is much less marked than in the case of sibs. If the mean M.R. of the children is, in fact, halfway between that of their mothers and that of the standard population, the implication would be that the population from which the fathers of these children were selected had a mean M.R. of 90·2. Inspection of the figures for mental ratios of mothers reveals that few of these patients have an M.R. below 50 and none below 38. The number of children born to people with I.Q. below 50 is very small, and a line drawn between 45 and 50 separates the infertile from the fertile group of defectives. It was in order to confine the analysis of regressions to the normal and the fertile group that the limit of M.R. 50 was decided upon (see Table XI).

Table XI. *Stanford-Binet tests: estimates for mean of general population based upon regressions of M.R. of relatives of patients with M.R. above* 50

Type of relationship	No. of pairs	Patients' mean mental ratio	Relatives' mean mental ratio	Estimate for mental ratio of general population
Female patient's child	77	64·8	77·5	90·2
Patient's sib	101	65·8	84·9	104·0
Patient's half-sib	37	65·1	85·6	95·8
Patient's nephew or niece	106	62·5	90·8	104·9
Patient's first cousin or equivalent relationship	68	61·0	89·2	96·2
All types	389	63·8	85·8	99·4

The half-sibs of patients are, on the average, less intelligent than the sibs but not greatly so. Half-sibs were most often to be found in families where there was illegitimacy and parents in such cases were not always up to the average for general intelligence. As usual, the relatives of idiots were more intelligent than the corresponding relatives of patients with M.R. above 50. In the case of half-sibs the regression should be three-fourths of the distance towards the normal, and the implication here is that patients with M.R. over 50 are drawn from a population whose mean M.R. is 95·8. The nephews and nieces of patients with M.R. above 50, on the same hypothesis of multifactorial inheritance of mental ability, could have been drawn from a population with mean M.R. 104·9.

The group of patients' first cousins was probably not a fair sample of such relatives in general for it contained an inordinately large number of children from one rural district. The mean M.R. of the population from which the patients and their first cousins were selected can be estimated on the assumption that the mean intelligence of cousins of the higher grade patients regresses seven-eighths of the distance towards the standard norm. Here the standard M.R. would be 96·2.

A weighted average for all types of pairs of high-grade patient and relative shows that for 389 pairs the regression is in close agreement with expectation. For the group of patients with M.R. of 50 or over, the ability of relatives is, on the average, such as would be expected if the general population had a mean mental ratio of 99·4 and if the genetic background of mental ability were multifactorial.

IV. ANALYSIS OF PORTEUS MAZE TEST RESULTS

With one or two exceptions, the effect of testing patients and relatives with the Porteus Maze test gave rise to figures quite similar to those which were obtained by using the Binet tests. In Tables XII and XV they are set out in detail with relatives classified by degree of relationship and by sex. For the high-grade patients, Table XIII is complementary to

Table XII. *Porteus Maze tests: mean mental ratios*

The patients are divided into groups according to mental ratio above or below 50; the relatives are classified according to degree of relationship.

Type of relationship	No. of pairs	Mental ratio of patient: Range	Mental ratio of patient: Mean	Mental ratio of patient: S.D.	Mental ratio of relative: Mean	Mental ratio of relative: S.D.
Female patient's child	23	0–49	39·1	6·7	71·5	17·8
	30	50+	69·6	15·7	81·6	18·2
	53	Total	56·3	19·0	77·3	18·4
Patient's sib	81	0–49	24·6	13·5	86·2	25·7
	73	50+	73·8	16·7	86·6	19·9
	154	Total	47·9	27·2	86·4	24·1
Patient's half-sib	10	0–49	34·3	8·0	86·3	20·9
	23	50+	80·1	18·3	80·9	24·8
	33	Total	66·2	26·5	82·5	23·9
Patient's nephew or niece	41	0–49	30·8	10·6	94·4	21·4
	89	50+	73·6	19·2	93·7	18·4
	130	Total	60·1	26·1	93·9	19·3
Patient's first cousin or equivalent relationship	69	0–49	40·9	10·5	82·8	20·7
	42	50+	70·5	13·2	87·8	20·4
	111	Total	52·1	18·5	84·6	21·1
All types	224	0–49	32·6	13·4	85·2	23·3
	257	50+	73·3	17·3	88·1	21·0
	481	Total	54·5	25·5	86·7	22·2

Table XIII. *Porteus Maze tests: estimates for mean of general population based upon regressions of mental ratio of relatives of patients with mental ratio above* 50

Type of relationship	No. of pairs	Patients' mean mental ratio	Relatives' mean mental ratio	Estimate for mental ratio of general population
Female patient's child	30	69·6	81·6	93·6
Patient's sib	73	73·8	86·6	99·4
Patient's half-sib	23	80·4	80·9	81·3
Patient's nephew or niece	89	73·6	93·7	103·7
Patient's first cousin or equivalent relationship	42	70·5	87·8	92·1
All types	257	73·3	88·1	97·5

Table XIV. *Stanford-Binet tests: mean mental ratios*

The patients are divided into groups according to mental ratio above or below 50; the patients and their relatives are classified according to sex.

		Mental ratio of patient			Mental ratio of relative	
Type of relationship	No. of pairs	Range	Mean	S.D.	Mean	S.D.
Male patient's male relative	59	0–49	31·0	11·2	89·5	20·0
	64	50+	63·9	10·8	89·1	17·5
	123	Total	48·2	19 6	89·3	18·2
Male patient's female relative	74	0–49	26·8	13·7	90·9	16·9
	72	50+	62·2	8·9	90 1	17·3
	146	Total	44·2	21·3	90·5	17·1
Female patient's male relative	77	0–49	36·1	13·3	85·5	19·2
	142	50+	64·1	11·5	83·0	16·8
	219	Total	54·3	18·0	83·9	17·6
Female patient's female relative	83	0–49	34·2	15·2	87·3	18·9
	111	50+	64·3	10·5	84 7	13·8
	194	Total	51·5	19·4	85·8	16·2
All types	293	0–49	32·2	14·0	88·2	18·7
	389	50+	63·8	10·8	85·8	16·5
	682	Total	50 2	19 8	86·8	17·7

Table XV. *Porteus Maze tests: mean mental ratios*

The patients are divided into groups according to mental ratio above or below 50; the patients and their relatives are classified according to sex.

		Mental ratio of patient			Mental ratio of relative	
Type of relationship	No. of pairs	Range	Mean	S.D.	Mean	S.D.
Male patient's male relative	39	0–49	29·7	10·1	95 9	20·5
	44	50+	72·5	18·2	91·2	23·2
	83	Total	52·4	24·7	93·4	22·1
Male patient's female relative	56	0–49	24·2	11·6	87·2	24·8
	51	50+	73·9	14·7	90·6	18·1
	107	Total	47·9	28·1	88·8	22·0
Female patient's male relative	55	0–49	38·9	13·3	87·1	20·6
	90	50+	72·5	19·5	87·9	21 0
	145	Total	59·7	23·8	87·6	20·8
Female patient's female relative	74	0–49	36·0	13 2	76·4	22·6
	72	50+	74·2	17·4	85·1	20·0
	146	Total	54·9	24·4	80·7	21·9
All types	224	0–49	32·6	13·4	85·2	23·3
	257	50+	73·3	17 3	88·1	21·0
	481	Total	54·5	25·5	86·7	22·2

Table XI, and the estimate of the mean M.R. of the general population, based on the weighted mean of all the regressions, is 97·5. The general result points towards the conclusion that the genetic background of ability to perform the Porteus Maze test is similar to that of ability to perform the Binet test. In a small group of half-sibs, the relatives had scarcely any more ability than the patients. The patients in this group were drawn from rather a specially inferior social class selected mainly by illegitimacy. For sibs the regression towards the normal is almost perfect.

V. SEX DIFFERENCES

In order to test various hypotheses about the influence of sex on the inheritance of mental ability, each pair of scores of patient and relative was arranged in one of four classes according to sex, in Tables XIV and XV. When all grades of patient were taken together, no difference could be found which was attributable to the action of sex-linked genes. The male patients had relatives who scored higher than the relatives of the female patients, but this might be partly accounted for by the predominance of low-grade male patients. Moreover, in the Binet results, the female relatives were slightly, but consistently better, and in the Porteus results slightly worse, than the male relatives. In both cases, the effect was independent of the sex of the patient. If genes located on the sex chromosomes were responsible for major variation in intellectual capacity, the male relatives of male patients and female relatives of female patients should have been markedly inferior to the rest of the relatives tested. There is no evidence of such a phenomenon in the Binet or Porteus test results even if low or high grade patients are considered separately.

VI. CONCLUSION

The Stanford-Binet test was given to 682 relatives of mentally defective patients and the Porteus Maze test to 481 such relatives. A method of scoring mental ratio was devised which enabled comparison to be made between the abilities of patients who were mostly adults and their tested relatives who were mostly children. In summarizing the results, the patients were classified according to their sexes and divided into two groups according to whether M.R. was below 50 or not. Relatives were classified according to sex and mental ratio. Each pair of patient and relative was considered as a separate datum although some patients had more than one relative.

The results agree with those of the larger survey previously published, in which mental ability was only rated, in that it was found that the relatives of patients with M.R. below 50 had better scores on the whole than the relatives of patients with M.R. 50 or above. Analysis of the two groups of patients separately, showed that familial cases in the low-grade, infertile group were rare, but when they occurred were usually low-grade. The ability of relatives of patients of M.R. 50 or over tends to regress towards the normal as would be expected if the factors underlying inheritance of mental ability were multiple additive ones.

Certain irregularities in the results can be interpreted as due to the selection of some of the institutional patients from social strata in which mental ability is more often subnormal than usual. No evidence was found of effects which could have been due to genes on the sex chromosomes which determine mental ability.

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TWO CONTRIBUTIONS TO THE THEORY OF MENTAL TESTING

I. A NEW PERFORMANCE TEST FOR MEASURING ABILITIES AS CORRELATION COEFFICIENTS

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I. *Introduction* (pp. 19–20).
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IV. *Description of the performance series* (pp. 28–30).
V. *Performance times* (p. 31).
VI. *Ability measured by correlation coefficients* (pp. 31–33).
VII. *Conclusion* (pp. 33–35).
References (p. 35).

I. INTRODUCTION

THE present paper continues those in which I have offered an introduction to Q-technique (1, 2), that is, the factor study of *persons* as variables instead of as items in a statistical population of individuals. In the present paper and one to follow it, I propose to describe two applications of Q-technique to mental testing, one of a particular nature concerning a novel way of measuring ability in terms of correlation coefficients, and the other of general and practical applicability, in which the various qualitative features of a person's responses in a performance test are used as a statistical 'population' for correlational purposes and factor analysis.

The setting for the first of these two applications, which is the concern of the present paper, is briefly as follows. Measurement of abilities has hitherto depended upon statistical populations of persons, either for seeking *average* performances (as in the case of standardization of a test like the Binet), or for supplying norms, or standardized scores. Now Q-technique has no necessary dependency upon any population of persons, and I have therefore asked myself whether ability might be measured in a systematic way in terms of only *one* person's ability, much as length is measured in terms of a standard yard. The thesis to be maintained in the sequel is to the effect that if a standard ability is taken, such as that of say an Einstein, other abilities might be measured in terms of it as correlation coefficients. The performance test I have devised is a solution, however crude in nature, of the question I set myself.

This application, however, serves in addition an ulterior purpose, concerned with the general theory of mental testing and factor analysis. Work on the correlation of persons has been conceived by Burt(3) as merely an alternative procedure to that of correlating tests or traits, it being a matter of convenience whether we correlate persons or tests. According to Burt, if there are more persons than tests, then the tests should be correlated, but if there are more tests than persons, then the persons should be correlated instead. In my opinion far more than mere convenience is involved in whether we correlate persons or tests. There are conditions under which it is necessary and sufficient to correlate persons, and other conditions when it is no less incumbent upon us to correlate tests. Some of these conditions are to be examined elsewhere(4), but others of a particular nature deserve consideration, and I propose to draw attention to these in the course of the present paper and one to follow it.

II. PREMISSES

Present-day mental tests are essentially *variables*, depending upon and varying with respect to a population of persons. There are obvious and essential differences between variables on the one hand, and statistical populations on the other; each involve assumptions peculiar to themselves. Thus, if I am working on verbal intelligence I do not dip my hand into a bag of verbal tests and select a number at random—I construct the tests I want, with an eye to certain psychological hypotheses that I wish to experiment upon. But I *do* select the necessary statistical sample of persons in some such random way. Under conditions of this kind nothing is gained, and no purpose or intention is changed, if I merely reverse the respective roles of persons and tests on grounds of expediency, even supposing for the moment that for the sake of convenience I could so change a variable into a unit of a statistical population, and units of a population into chameleon-like variables. The very verbal tests owe their *form* and method of construction to the exigencies of psychological theory on the one hand, and to the needs of a statistical regard of persons on the other. We must take care, therefore, to see that these purposes and principles are not just conveniently overlooked when we seek to correlate persons instead of tests, and tests instead of persons.

If the technique of correlating persons has to have its own place in mental test science, or in psychometry in general, it must set its own conditions and be based on assumptions peculiar to itself. These must be of such a nature that it is necessary and sufficient to use persons as variables, and tests or qualities themselves as units of a statistical population. Moreover, the psychological hypotheses should relate specifically to the *persons*, whilst purely statistical conditions should govern the tests or qualities. It is for conditions and principles of this kind that I have looked in my regard of Q-technique.

In normal mental test theory there is a test variable (consisting, if need be, of several sub-tests which are used additively) in terms of which each person obtains a score; these scores are usually approximately normally distributed, and are standardized—that is reduced to values with mean 0, and standard deviation 1. One test is then correlated with another to see how far the two may be presumed to measure one and the same ability; or many tests are intercorrelated, with a very similar objective. These tests need satisfy no statistical conditions such as pertain to a universe from which samples are drawn. Indeed, on occasion there might be only *one* test of a particular kind in existence, and yet obviously it could satisfy all the necessary conditions of a variable with respect to a population of persons. Thus, two tests of completed and interrupted tasks, of the kind devised by Zeigarnik (5), cannot be applied to one and the same group of persons—for as soon as they have performed the first test they know what to expect in the second, and this completely alters the nature of the latter. In effect only one test of this kind can ever exist for a given population of persons. Yet valuable psychological work might be done with this one test variable.

As for the statistical population of persons, its constituents are considered to be discrete and separate entities. But many assumptions enter into the grouping of human beings into a sample; it is in fact notoriously difficult to obtain a satisfactory statistical population anyhow or anywhere. Thus it is not permissible to include Negroes, Chinese and Whites into one person-population; nor would the psychologist care to mix children from London with those from Leeds. Physically the individuals are separate, but psychologically they are complexly related. Persons are bound together by ties of race, environment, education, training, social and civic culture, and if any of these are too firmly knotted for a section of human beings, that section cannot be included in the universe of a population from which samples are to be drawn. The essential criterion for a statistical universe is that it should be possible to regard all its constituents as dependent upon innumerable

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small influences, interacting in infinite complexity, so that the conditions are produced for the theory of "errors" to apply. Such a theory can account for the normal distribution (or indeed for other distributions of this kind as well) of the quantities for which the statistical population of persons has been appraised or measured (6). As is well known, both Spearman and Thomson have considered conditions of this kind in seeking to account for the substructure of abilities (7).

Analogous conditions for ability measurement in terms of Q-technique would require us to contemplate *one* person, and *only* one if need be, as a variable, and a statistical population of tests of some kind from which samples could be drawn. For any such sample, each of the tests would obtain a score as a result of the individual's operation upon it; but these scores would not be added, any more than the scores for different persons are added when a population of persons is measured; instead they would have a more or less normal distribution in terms of some common qualitative and quantitative character. Their mean for this character would be zero (when standardized), and their standard deviation 1.

Even up to this point in the argument there exist no tests anywhere that would satisfy these conditions, unless only *one* kind of test is considered, involving one quality of performance, so that a common basis of quantification could be employed.

Moreover, for our special problem certain other conditions have to be satisfied. If one person is to be used as a standard in terms of whom to measure the ability of other persons by correlating these with the standard person's array; and if the correlations themselves are to be direct measures of these persons' ability, then the population of tests has to be of such a nature that only persons as brilliant as the standard can gain similar scores, test for test. Others less capable have to gain different scores for these same tests, test for test; and stupid persons have to obtain either random scores (relative to those of the standard person), or else turn tables upon the scores obtained by the capable ones—in tests for which the clever persons obtain a high score the stupid ones have to gain a low score, and vice versa. When the arrays of scores for the various persons are correlated with the standard array, only those persons as brilliant as the standard will correlate highly with him, and those less capable will correlate less with him, and less in proportion to their lesser abilities. Certainly no published mental tests known to me could be thrown together to constitute a population of tests fulfilling these conditions! It is obvious, too, that if such a test could be made it would have to be of a form unlike any at present known.

If it were indeed possible to construct such a population of tests it might well be asked *why* we should go to all the trouble of measuring ability in this way, as a correlation coefficient, instead of by the usual way. It is possible, of course, for us to set up all kinds of bizarre standards in terms of which to measure individuals. I certainly did not think of making any such purely arbitrary scales. Instead I hoped to conceive of a test with the same kind of systematic method and procedure attached to it that we expect in a science of mental testing. The new form of test, for example, has itself to supply individual differences in an orderly way, but *without any* necessary dependency upon statistical populations or regard of persons. To achieve this is at the moment a matter of academic interest only, but it is important to pay regard to the possibilities. So much is taken for granted in current mental test theory that everything is to be gained by questioning the assumptions involved, and perhaps still more by experimenting with others. And this, indeed, is the essential purpose of the new test. It will serve to demonstrate that Q-technique can have its own special problems, and that its assumptions, principles, and objectives can be peculiar to itself, and independent of those involved in present-day testing theory. For the present it is immaterial whether the test is practical or impractical; it is merely incumbent upon us to think along any new lines that Q-technique may suggest to us.

III. PRINCIPLE OF THE NEW PERFORMANCE TEST POPULATION

Many test populations of the required kind could be devised. But for the purpose of the second paper I need some performance tests, and I have therefore made the following test of a performance kind (I shall call it as a whole a *test*, but it is really conceived as a sample of tasks, drawn from a universe of similar tasks that could be made). It will be remembered that an illustrative rather than a practical matter of mental testing is under consideration, and it is in no way intended that the following test should be used for work-a-day measurement of ability.

The performance test has the following construction. It consists of twenty tasks, each like a large-scale jig-saw puzzle. The individual has to make twenty copies of a standard shape from given pieces; these copies are not exact duplicates of the standard, but may diverge considerably from it (see Fig. 1). Having constructed the twenty copies under time-limit conditions *the individual is asked to appraise them for their degree of likeness to the standard. All the copies have this character in common, i.e. some degree of similarity to the standard; it is this that affords a basis for their quantification.*

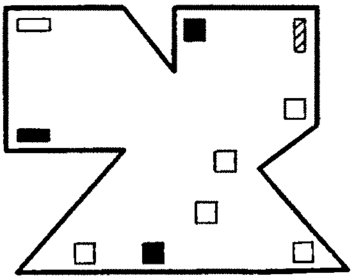

Fig. 1. STANDARD.

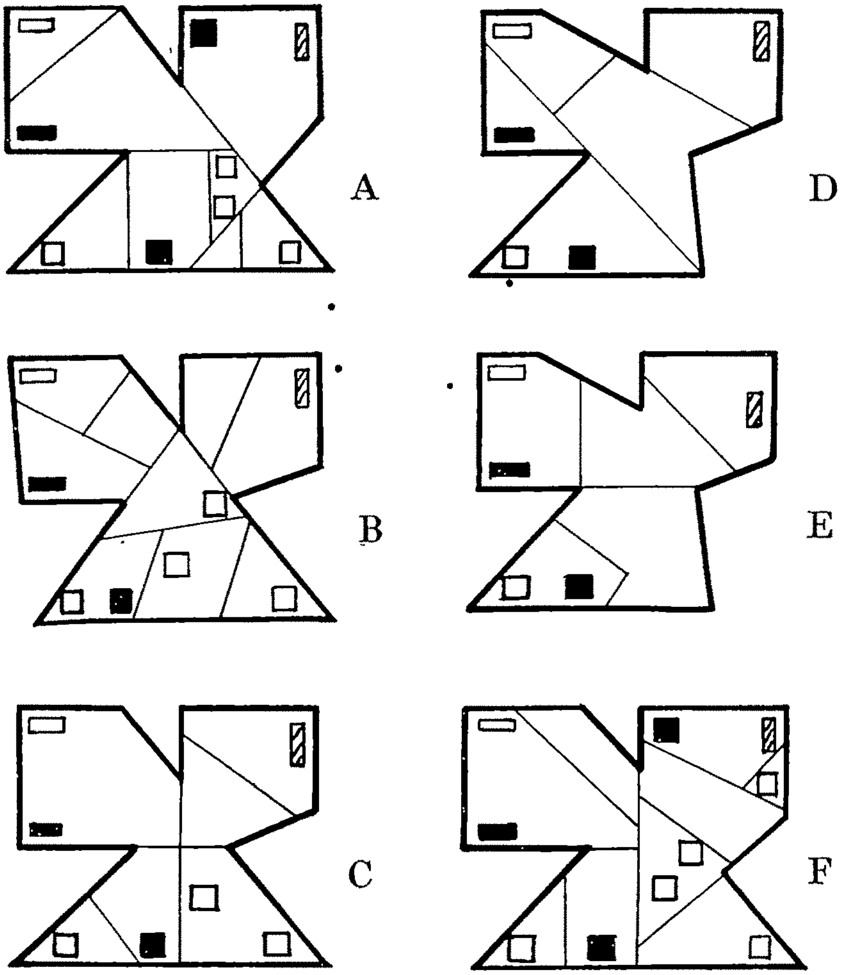

Fig. 1. A—F.

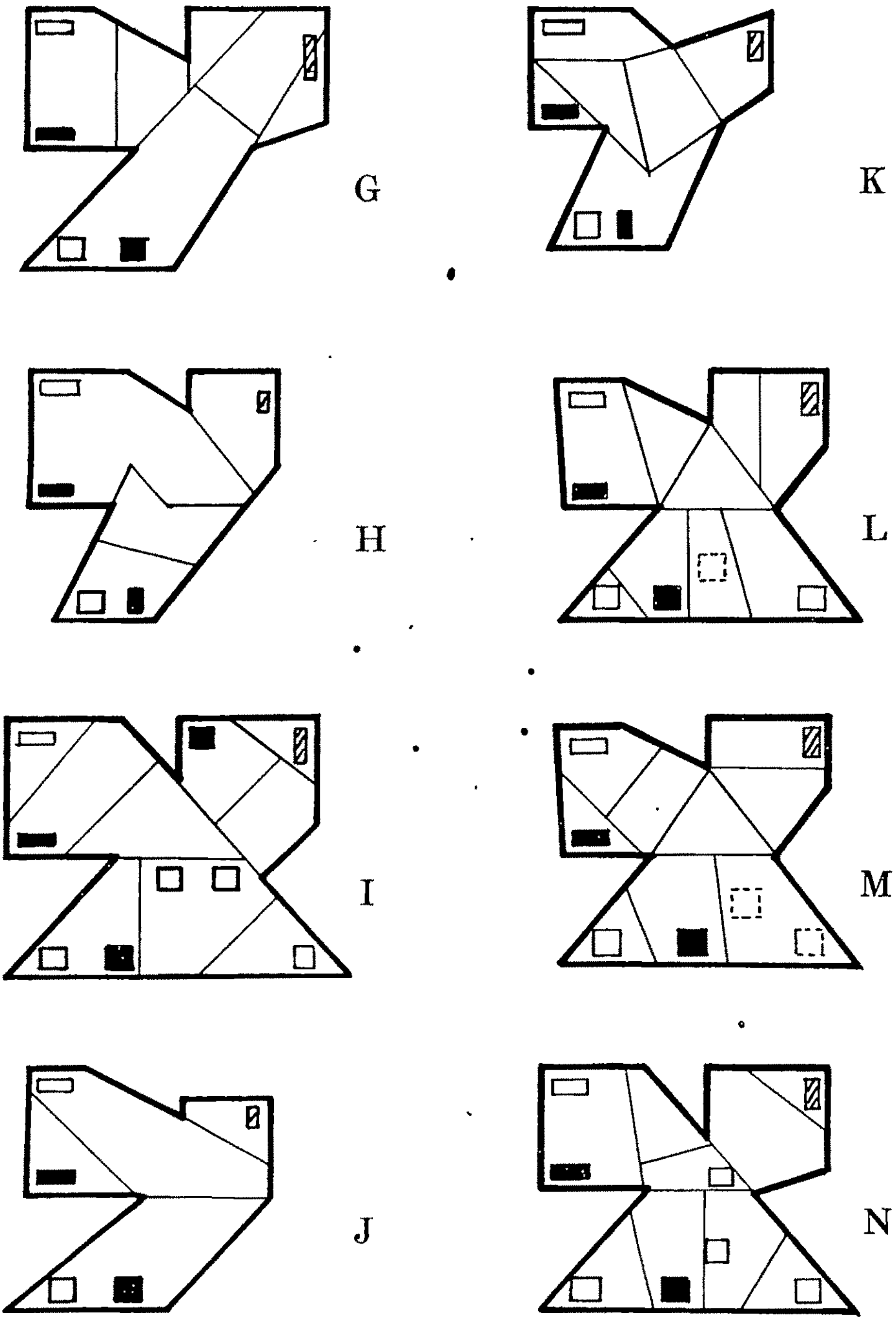

Fig. 1. G–N.

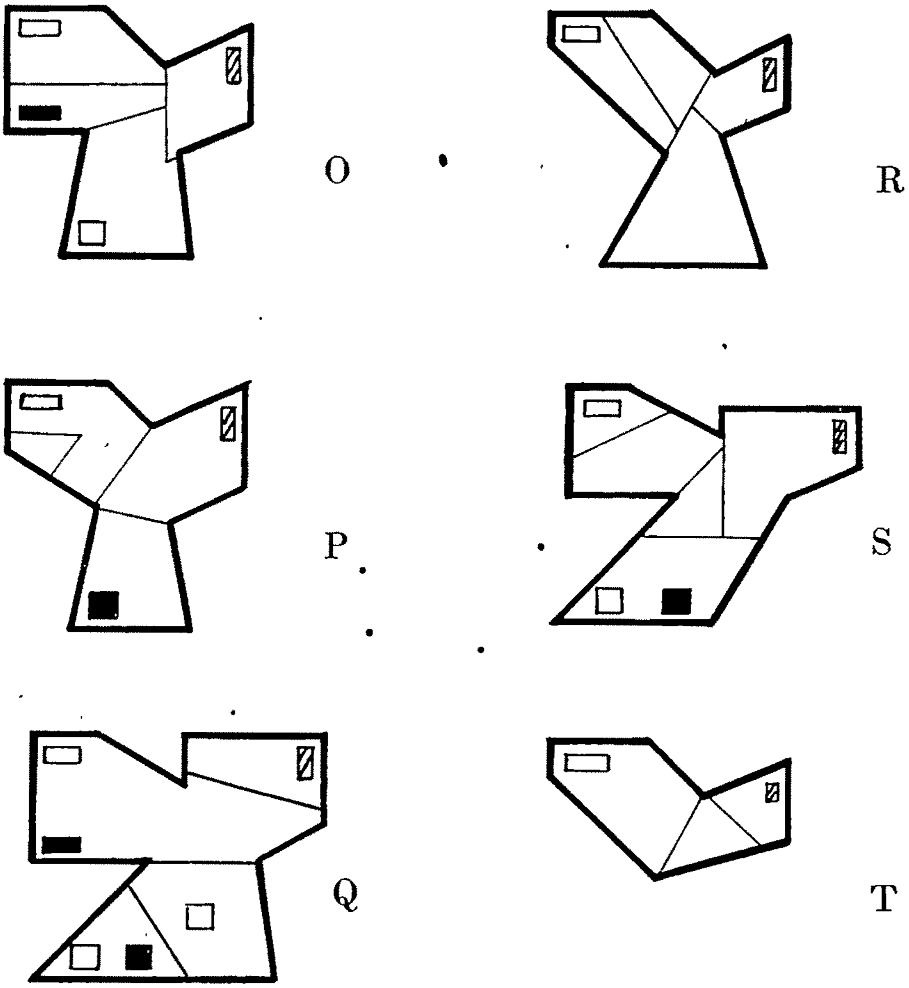

Fig. 1. O–T.

Fig. 1. Showing the standard shape and the 20 completed copies A to T, in order of production. Faint lines mark off pieces composing the copy; thus A has 8 pieces, and B to T, 9, 6, 5, 5, 9, 6, 4, 9, 4, 6, 9, 9, 9, 4, 4, 4, 4, 5 and 3 pieces respectively. The copies depart from the standard in the order F, A, I, B, N, C, L, M, Q, D, E, G, S, J, H, K, O, P, R, T. Full black squares or rectangles are *red*, empty ones *blue*, hachured ones *green*, and dotted ones *bright (light) green*. The latter colour is not on the standard. All drawings are to scale 1:7.

Now only twenty tasks are involved. But two or three hundred could be made if need be, and for a large number like this the appraisal for similarity to the standard would supply a frequency distribution of the quantities, and the distribution could theoretically and practically be approximately normal in shape. Only a few of the productions would be very similar to the standard; only a few very little like it; and most more or less like it. (There are other conditions, as we shall see, that would ensure this.) For my small sample of twenty tasks only, however, I shall use a rank order. The copy most like the standard will be placed first in the rank, and that least like it last, with the others duly in between in order.

Now if the twenty tasks are correctly made there is a correct order in which they can be placed when appraised for similarity to the standard. But if the individual makes errors in constructing his copies, or does not finish them in time, the result will be that he cannot supply the correct rank order. Moreover, only really capable persons can solve the tasks in the given time, and therefore they alone have the opportunity of ranking their productions correctly; and the less capable the person the more upset is his rank order relative to the standard, correct one.

This comes about in several ways. Persons of poor ability cannot construct the more difficult copies of the standard in the allotted time, and the more difficult the task the more in fact it resembles the standard model. Thus when such a person ranks his productions those that ought to have been placed high up in the rank order are of necessity placed lowly; and the less capable the person the more lowly will he place the tasks which are, when correctly solved, most like the standard model. On the other hand, these persons usually manage to construct certain easy copies, which are superficially very similar to the standard, and these are therefore very readily placed first in their rank order! Similarly throughout the sample of tasks there are sources of error which result in displacements of this kind, with the net result that the more stupid the person the more he tends to place the *easiest* tasks (which are really least of all like the standard) at the top of his rank order.

Now when the rank orders of different persons are correlated the desired result is obtained: clever individuals correlate highly with the standard rank order, and dull ones correlate less highly with it, and the more dull they are the less highly do they correlate with the standard array. Compared with existing forms of mental tests this one seems to turn things completely inside out, like a sausage with its skin on the inside. But the test as a whole does what is required, as the following fuller description of it, and a set of results obtained by its use, will show.

I would again remind the reader that although only twenty tasks are involved in the present sample a very much larger sample could be used if need be. Any such large sample would have its own *correct* frequency distribution in terms of which to compare those supplied by each individual for the appraisal of his productions for their likeness to the standard model.

IV. DESCRIPTION OF THE PERFORMANCE SERIES

The standard model is placed in front of the subject to be tested. Twenty copies of this have to be made by him from twenty sets of pieces. Any one set consists of from three to nine pieces. Only one of the sets provides, if it is correctly put together, an exact copy of the standard model, the rest diverging from it progressively, and the subject is asked to make as exact a copy of the standard as he can with the given pieces of each set. The copies that he *can* make are objectively determined by the given pieces, and if the subject works deliberately and is prepared to construct his copies somewhat imaginatively it is possible for him to arrive at a conclusion satisfactory to himself and to the test constructor. The twenty copies he is expected to make are shown in Fig. 1 (A to T). The pieces (one set at a time) are given to the subject in random order, with reverse sides uppermost; they have to be turned over and placed in juxtaposition, much as dominoes are placed side by side on a table, in such a way as to provide the required copy of the standard.

All pieces are made of ¼ in. soft pine, varnished and numbered on the reverse side, painted white along all edges, and enamelled a bright lavender colour on the face. Sizes are indicated in Fig. 1, which shows the pieces drawn in proportion, in the positions they take up when the copies are 'correctly' put together. The standard and various pieces of most sets have painted upon them a number of coloured patches, red and blue squares, and red, blue and green rectangles of a regular size throughout. These, and the shapes of the pieces, offer the clues that are needed for solving the copies.

The order of application of the sets is as shown in Fig. 1 from A to T: it will be observed that the copies diverge progressively from the standard (although not in the order of application of the sets) as we pass from A to T: the first few are not unlike it, but those towards the end of the series are only abstractly like it. The copies actually diverge in a logical way according to a very simple principle of decretion—colours are omitted and sections cut away from the standard shape; this logical order supplies the series given in the footnote to Fig. 1. After performing the first few sets the subject is expected to have learnt that the copies he can make fall into some such series.

The subject sits at a small table with the standard model in front of him, inclined on a book-rest at the back of the table. A smooth-surfaced strawboard (12 × 10 in.) is placed on the table in front, having upon it the pieces of a set, all of which, to begin with, are face downward, each piece having a separate (random) position. The following instructions are given before the individual begins:

"These pieces (set A), when turned over (demonstrate with one piece) can make a copy of this standard model (point). This set of pieces makes almost an exact copy of the standard when the pieces are correctly placed side by side; but the later sets do not make exact copies of the standard. You will be asked to make twenty copies, from twenty different sets of pieces, of which this set is the first.

"The first few sets make copies that do not differ much from the standard, but the later sets may differ considerably from the standard. Nevertheless, if you follow all the clues given on the pieces you can nearly always construct a model something like the standard. In each case you have to construct a copy which will be as nearly like the standard as it is possible to make it with the given pieces.

"With these pieces, then, see how quickly you can make a copy of this standard, as nearly as possible like the standard, using all the given pieces."

The individual's construction is left on its strawboard base, and is lifted bodily, without disturbing it, on to a low table at his side. The next set of pieces is then placed before him, and the testing proceeds until all twenty copies have been attempted. The time taken to construct a copy is recorded by the tester; and if a copy is not finished at the expiry of five minutes the subject is stopped from working at it. It is placed along with the completed copies in the state of construction reached by the individual.

Once they are correctly completed, the copies only rarely fail to satisfy the individual that they are the required ones. This is at first sight not an obvious feature of the test, because in most performance tests the pieces fit into recesses and this provides an automatic check on the correctness or otherwise of a production, whereas in the present case there are no recesses and the copies themselves are somewhat imaginatively conceived. The individual has to decide for himself, sometimes very explicitly, whether or not his productions 'will do'. He is helped in this by the 'wholeness' of the finished copy, and by the objective fact that the pieces 'fit together nicely'. The need to make a decision about a copy, however, is very often apparent. Otherwise the test, so far, is like any other of a performance kind, except perhaps that it embraces a more varied sample of intelligent activities—it entices the individual into many avenues of performance and thought. The test seems to involve, amongst other things, a certain need for inventive or imaginative use of material, orderly planning, making decisions, analysis and synthesis, space-sense (8), generalization, learning by experience, and the ability to execute a lengthy series of interconnected constructions. These qualities make it, on its own account, a test of no mean difficulty.

As in the case of any other test of this kind, the *time* it takes the individual to perform each task, or the number he successfully constructs in the allotted time, may be used as a measure of ability.

The second, and for our present purpose the most interesting, part of the testing now takes place. When all the sets have been attempted, the subject is asked to appraise them for their similarity to the standard (which he can carry about in his hand, and look at as often as he likes). That most like the standard has to be placed first, that next, second, and so on in rank order. In the case of unfinished constructions the individual is told that they are probably as satisfactory as any that could be made with the given pieces, and that he should judge them in that light.

As I have already indicated, only one copy can be made which is a complete duplicate of the standard. This is copy F in Fig. 1. The other copies diverge from the standard either with respect to shape, colours, or size. Thus, copies A and B are identical for shape and size, but one lacks a coloured square that the other has got, and it is to that extent logically less like the standard. There are differences of this kind between all successive copies, in terms of which one can decide objectively, at least on logical grounds, that one construction is more like the standard than another. This is particularly the case, of course, when incomplete constructions are involved—i.e. those that the subject has been unable to complete in the given time. Needless to say, judgements are rarely based on logical grounds alone—on the contrary most individuals seem to appraise the copies by 'general impressions', or even on aesthetic grounds. Only the most careful, and capable, subjects seem to appraise the copies rationally; and the more stupid the individual the more likely he is to misplace the copies due to inadequate analysis. Copies C, D, and E are very apt to be placed higher up the rank order on this account than their logical analysis would warrant.

I have available the results for forty students to whom I applied the new performance test, and these data should be sufficient to illustrate the test's uses. The students were of an age, within two years, and were all drawn from various Colleges at Oxford. For each we have (i) the time taken to complete each of the twenty copies, with a record of errors and unplaced pieces, and (ii) his rank order for these twenty copies, appraised for their similarity to the standard.

V. PERFORMANCE TIMES

I propose to use the time taken by the subject to complete his copies (with a correction for errors or unplaced pieces) as a straightforward measure of ability in the test. That is, the person who can perform the tasks quickest will be considered to be the highest in the scale of ability in this test. This measurement can then be used as a basis of comparison with ability as measured in terms of a correlation coefficient. The time measure is therefore only of indirect interest to us, but a few details are necessary.

Table I shows the arithmetic means of the times taken by the forty students to solve or attempt each of the twenty copies A to T. Sets A and B take the longest time, and D and H the least. There is a tendency for the time, and the difficulty of the tasks, to decrease as we pass from A to T. Account has to be taken of errors and incomplete solutions, and a preliminary study indicated that it would be fair to add 50 sec. to the subject's time at a copy, for each piece left unplaced or incorrectly placed. With this correction the mean times per task are those given in column (*a*) of Table I. These corrected times will be used as straightforward measures of ability. In point of fact the test calls for high ability, and a study with other subjects has shown that these corrected times correlate $-0{\cdot}738 \pm 0{\cdot}076$ (S.E.) with the results of a pencil and paper intelligence test. The reliability of the test, gauged by the Spearman-Brown formula in terms of these time scores, is 0·893 for the forty students.

Table I. *Showing average time for forty students to perform each item. Column (a) gives the same averages with a correction added for unplaced and misplaced pieces. (All times to the nearest second)*

Test item	Average time	(*a*) Corrected average	Test item	Average time	(*a*) Corrected average
A	230	296	K	100	113
B	231	278	L	215	292
C	148	177	M	192	229
D	52	58	N	201	267
E	110	120	O	95	105
F	157	183	P	66	74
G	132	140	Q	119	127
H	49	54	R	116	148
I	135	146	S	89	93
J	106	114	T	73	73

VI. ABILITY MEASURED BY CORRELATION COEFFICIENTS

Our crucial concern is with each subject's appraisal of his productions for their similarity to the standard model. This similarity supplies the needful quantitative basis in terms of which individuals can be correlated in turn with the rank order given by an expert performer to his own productions, and it is anticipated from the nature of the test as a whole that these correlations will be in proportion to each person's ability, approximately, as this is assessed by more usual methods.

That is, we require an expert to appraise his own productions; and the rank order that he supplies will thereafter be used as a standard array in terms of which any other person's ability can be measured. He will set an upper limit to the scale of ability, but this will be no practical disadvantage if the expert is sufficiently competent. All other persons will be scaled downwards from this standard array, for no one can correlate more than 1·00 with it, although many might correlate perfectly with it. In the present case it seems that the logical order of decretion, upon which the test was originally constructed, would not differ greatly from the rank order that an expert analytical thinker and performer would supply, and I therefore propose to use this order as the standard array. The order in question is that given in the footnote to Fig. 1.

Each student's rank array was therefore correlated (rank coefficients) with this standard array, and the results are shown in Table II. The correlations range from 0·99 to 0·52, with a mean 0·870, there being some skew towards perfect correlation. Lower correlations would result all

Table II. *Showing frequency distribution of rank correlations. The correlations are between the logically correct order and the subject's own order for his productions*

Correlation	No. of students supplying this correlation
1·000 to 0·990	1
0·989 to 0·980	2
0·979 to 0·970	1
....	
0·969 to 0·950	4
0·949 to 0·930	6
0·929 to 0·910	3
0·909 to 0·890	5
0·889 to 0·870	4
0·869 to 0·850	3
....	
0·849 to 0·810	3
0·809 to 0·770	3
0·769 to 0·730	1
0·729 to 0·690	1
0·689 to 0·650	—
0·649 to 0·610	2
0·609 to 0·570	—
0·569 to 0·530	—
0·529 to 0·490	1
	Total 40 students

round if the time allowed per copy were less than the five minutes allowed as maximum in the case of these students. Under the conditions obtaining for these forty students, all of whom were men, one or two women have supplied a correlation as low as 0·33 with the standard array.

It is these correlations that we hope will be proportional, approximately, to ability as measured in the more usual way. An indication of their success in this direction is afforded by using them as scores, and seeing how far they correlate with, say, the time-scores referred to in the previous section. The two arrays for the forty subjects (their time-scores for producing the copies in the first place, and their correlation with the standard array just mentioned) correlate −0·768 ± 0·044 (S.E.) (product-moment correlation). That is, the person who performs the twenty tasks in the *quickest* time (corrected for errors) tends to have the *highest* correlation with the standard array. To this extent, therefore, our original anticipations are fulfilled; and indeed it is quite obvious as one sees the performances and subsequent appraisals, that only the more competent subjects can supply a rank order that approximates adequately to the logically correct order. The correlation −0·768 is linear, so that within the limits of this sample of twenty tasks the correlational measures of ability are also linear. Otherwise they would seem to involve the usual conditions of any correlational surfaces.

VII. CONCLUSION

It has been shown, then, that a sample of tests can be made in terms of which one individual at a time can be correlated with the *ability* of another person, whose expertness is accepted as a basis of measurement, and that the correlations so obtained are themselves a direct measure of ability—at least to the extent shown by a correlation of 0·768 with ability as measured in the usual way by the *time* it takes an individual to solve the same tasks. The time-scores, and the appraisal arrays, reach very similar ends.

Several matters of theoretical interest are involved, and it was to illustrate these that the test has been made. It has been my object to demonstrate that whether we correlate persons, or tests, should not be thought of as merely two interchangeable ways of treating one and the same matrix of data. If Q-technique, the use of persons as variables, is to be of any interest and importance on its own account, it must seek new fields of endeavour in psychometry. Beginning from the fact that modern mental test theory and practice is based on the use of tests as *variables* and persons as a statistical population, I have asked myself whether these roles might be reversed. Prof. Burt (8) has suggested one way of effecting this, but it is scarcely the kind of solution or point of view that I have in mind. It leads to no new principles or underlying premisses in mental test theory. According to Burt it is merely a matter of convenience whether we correlate persons or tests. But the purpose and function of a door is not changed by hanging it back to front; and what I want of the concept of correlating persons is a new door, or better a window, and not a badly hung back-to-front door.

The use to which I have put Q-technique in the present paper accepts the conditions of a person as variable: if need be, for instance, we could vary *one* individual, testing him when he is drunk, sober, drugged, or depressed, and determining the influence of these conditions on his ability as measured relative to a standard ability. The study also accepts the implications of a statistical population and regard of *tests*. It is not sufficient, in this connection, to dip into a catalogue of tests for our sample populations. Statistical populations of use to psychology are not so much random, as infinitely complexly related in their components. The sample of tasks used above has been based on such considerations; a number of productions have to be appraised for their similarity to a standard model, and it is theoretically possible to construct a universe of such productions, distributed normally for their degree of likeness to the standard model. The small sample used here is but a faint echo of this universe, of course. Under these conditions I have next asked whether it is possible to dispense with individual differences as the *basis* of measurement of abilities, and whether the use of statistical populations of persons is essential for this measurement. No statistical population of persons, or gross averaging of samples drawn from such populations, is anywhere involved in the theory of the above test population. It is *sufficient* to correlate persons one by one with the known standard in order to arrive at a methodical measurement of their abilities. The individual differences between persons issue from the testing as a consequence of the logical structure of the test material, and not because of any dependency on, or statistical formulations about, persons. In short, a new principle of systematic quantification of abilities is contemplated. Finally, to arrive at the necessary individual differences a new *form* of test construction has had to be devised, which makes use of principles (or tricks if you will) which are specific to the needs of a Q-technique approach to the measurement of ability. It is not suggested for a moment that we should in practice seek to measure abilities as correlation coefficients: but if we were driven to it, it could be done.

In all this, Q-technique is reaching out towards its own place in the sun, towards its own particular functions in psychometry. It searches for conditions under which the correlational regard of persons, as variables, can have a unique place in mental test theory and practice. There is nothing merely permissive, no matter of convenience only, in this regard of persons who are correlated—on the contrary the very foundations of mental test theory are uprooted and examined, and fresh young trees are planted in their place.

I shall discuss other matters of theoretical interest pertaining to the above performance test series in a subsequent paper.

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INDIVIDUAL DIFFERENCES IN AMPLITUDES OF OSCILLATION AND THEIR CONNECTION WITH STEADINESS OF CHARACTER

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I. INTRODUCTION

IN a previous investigation (5) it had been concluded tentatively that the amplitude of a person's oscillations in efficiency during a short test was associated with his steadiness of character. The results were however diminished in value by the fact that the methods of obtaining some of the data did not permit of a statistical treatment which would give trustworthy estimates of significance.

The present investigation has improved on the former by having

(1) a larger number of subjects,

(2) a wider range of tests,

(3) statistical methods giving estimates of the significance of the results.

II. PROCEDURE

Ninety subjects were available instead of fifty-five, and seven tests were used instead of four. These included the previous four tests, viz.:

(1) Crossing out digit pairs with sum 10.

(2) Crossing out digit pairs with difference 3.

(3) Writing letters of the alphabet.

(4) Writing cardinal numbers backwards.

And also

(5) 'Code' test, replacing each letter on a sheet by the one two places before it in the alphabet.

(6) Crossing out pairs of consecutive digits.

(7) Crossing out permutations of the same 4 digits.

Each test was applied twice, but only one testing per day was done with any one class. The procedure in giving the tests was the same as before. In tests (1), (2), (6), (7) the subjects had to cross out by a stroke \ the specified groups, and by a stroke / the other groups. Correction by a second stroke was allowed, giving ×, but this occurred rarely with most subjects. The specified pairs were scattered by chance drawing among the others in the ratio of 1 to 4, to avoid any rhythmic effects. The other three tests were of the 'production' type, involving writing on the part of subject. In each test 'Stroke' was called every 15 sec., and a vertical stroke was made to mark the place reached on the paper. Twenty periods were taken for each test, giving a total time of 5 min.

The measure of the amplitude of oscillation adopted was the same as previously, viz.:

$$\text{Amplitude ``0''} = \frac{\text{Standard deviation of scores in 20 periods}}{\text{Mean score in 20 periods}} = \frac{\sigma}{M}.$$

Fatigue effects tending to lower the score towards the end of test were again found to be absent (see (5), p. 184) and no adjustments were made on that account.

The reliabilities of the tests were obtained by calculating the correlation coefficients between the two applications.

III. FREQUENCY DISTRIBUTIONS

The frequency distributions of the coefficients for amplitude of oscillation (σ/M) were investigated diagrammatically, and also by comparison of the observed frequencies with those to be expected from a normal distribution. The diagrams showed the familiar 'cocked hat' shape which is usually considered sufficient warrant for the use of the product-moment method of calculating correlations and their probable errors. The coefficient σ/M is not subject to any particular limiting value. The observed and calculated frequencies are given in Table I.

The observed frequencies are not widely different from the normal frequencies. Using the χ^2 criterion (2) to examine the amount of the difference, it is found that none of the distributions is significantly different from a normal distribution.[1]

Table I

	Range of σ/M					
	$M'-3\sigma'$	$M'-2\sigma'$	$M'-\sigma'$	$M'+\sigma'$	$M'+2\sigma'$	$M'+3\sigma'$
Calculated frequencies	1	11·5	27	27	11·5	1
Observed frequencies: Test 1	0	15	31	20	10	3
Test 2	1	10	34	17	14	3
Test 3	2	13	29	23	7	5
Test 4	1	17	25	24	9	3
Test 5	1	9	35	19	13	2
Test 6	2	16	17	29	13	2
Test 7	0	11	32	28	4	4

M' and σ' refer to the distribution of σ/M.

The frequency distributions for Ages, I.Q.'s, and Mean Scores were also investigated diagrammatically, and the corresponding figures, with expected values, are given in Table II. These three distributions are more nearly normal than the previous ones. The age distribution is approximately normal because the classes available happened to be one first form, two second forms, and one third form.

Table II

	Range					
	$M-3\sigma$	$M-2\sigma$	$M-\sigma$	$M+\sigma$	$M+2\sigma$	$M+3\sigma$
Calculated frequencies	2	11·4	28·6	28·6	11·4	2
Observed frequencies (age)	2	15	26	27	12	2
Observed frequencies (mean score)	4	12	26	28	9	5
Calculated frequency (I.Q.)	1·7	10·6	26·2	26·2	10·6	1·7
Observed frequency (I.Q.)	3	14	24	21	10	5

IV. RELIABILITIES

The above considerations justify the use of the product-moment method of calculating reliabilities and inter-test correlations. The reliability correlations are given in Table III. The coefficients vary between 4 and 10 times their probable error, and must therefore be taken as significant.

Table III

	Test						
	1	2	3	4	5	6	7
Correlation between 1st and 2nd applications	0·48	0·54	0·36	0·475	0·39	0·56	0·39
P.E. of r	0·06	0·06	0·08	0·06	0·07	0·05	0·07

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[1] In applying the χ^2 criterion, columns 1 and 2 were taken together, and columns 5 and 6, giving four classes only.

V. AGE AND INTELLIGENCE

In the previous investigation no account was taken of intelligence, as I.Q.'s were not available. For the present tests I.Q.'s were known for most of the subjects.[1] The mean values of σ/M from the two applications were found for each test, and the correlations between each test and I.Q. calculated. The correlations are given in Table IV.

Table IV

	Test						
	1	2	3	4	5	6	7
r (with I.Q.)	0·06	0·04	−0·12	0·09	−0·04	−0·005	0·08
P.E. of r	0·08	0·08	0·08	0·08	0·08	0·08	0·08

Mean value of $r=+0{\cdot}015$.

Hence it seems reasonable to assume that the measure of oscillation amplitude used is almost independent of intelligence level. In corroboration of this, the correlation between the Mean Score for all the tests, and I.Q. was $r=0{\cdot}012$ (P.E. 0·078).

The connexion between Mental Age, as found from the intelligence test results, and the oscillation scores was also investigated, giving the correlations shown in Table V. The correlations for tests (1), (3), (5) are significant. The correlation between Mental Age and the Mean Score from all the tests was $r=-0{\cdot}284$, P.E. $=0{\cdot}074$.

Table V

	Test						
	1	2	3	4	5	6	7
r (with Mental Age)	−0·27	−0·08	−0·29	−0·06	−0·42	+0·06	−0·04
P.E. of r	0·07	0·08	0·07	0·08	0·07	0·08	0·08

The correlations between age and the mean score for each test were found to be as shown in Table VI. The correlations for tests (1) and (3) are significant, and for (4), (5) and (7) probably significant (i.e. probability of chance occurrence is $<0{\cdot}1$ (2)). They tend to confirm the result suggested in the previous investigation of an inverse relationship between oscillation and age for the range of ages tested. Previously the correlations for writing digits (test 4) and writing letters (test 3) were insignificant. One is now significant ($r=4$ times P.E.) and the other probably significant ($r=2\frac{1}{2}$ times P.E.).

The correlation between the Mean Score in all the tests and age is $r=-0{\cdot}295$, P.E. 0·067, which is significant.

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[1] The Simplex Group Test was used. The average I.Q. was about 109·7. The average for the school as a whole was, on several occasions, found to be almost exactly 100, and the distribution very close to normal. The present subjects were from classes taking a full academic course, and hence above the general average.

Table VI

	Test						
	1	2	3	4	5	6	7
r (with age)	−0·37	−0·065	−0·29	−0·19	−0 21	+0·026	−0·18
P.E. of r	0 06	0·07	0·07	0·07	0·07	0·07	0·07

VI. INTERCORRELATIONS AND A GROUP FACTOR

The intercorrelations of the seven tests, using the mean value of σ/M for each, were found for those subjects, numbering seventy-nine, who had completed the fourteen applications. The twenty-one coefficients obtained were all positive (previously some were negative but not significant). Fourteen of them were significant ($r = 3\frac{1}{2}$ times P.E.) and the lowest of the other seven was $1\frac{1}{2}$ times its P.E. When these coefficients were corrected for attenuation, using the reliability figures from Table III and the formula

$$r_{AB} = \frac{\sqrt{(r_{A_1B_2} \cdot r_{A_2B_1})}}{\sqrt{(r_{A_1A_2} \cdot r_{B_1B_2})}},$$

the values ranged from 0·33 to 0·90.

The intercorrelations were also found after the effect of age had been eliminated by the use of the usual formula for partial correlations. The figures obtained were little different from the raw coefficients, and were still all positive, with thirteen significant. The values are given in Table VII where the raw correlations are in the lower half, the age corrected figures in the top half in italics.

Table VII

	Test						
	Adding digits	Sub-tracting digits	Writing letters	Writing numbers	Code	Con-secutive digits	Rear-ranging digits
Adding digits		*+0·288*	*+0·314*	*+0·206*	*+0·185*	*+0·401*	*+0·306*
Subtracting digits	+0·291		*+0·205*	*+0·213*	*+0·366*	*+0·341*	*+0·108*
Writing letters	+0·388	+0·215		*+0 426*	*+0 510*	*+0·206*	*+0·172*
Writing numbers	+0·258	+0·221	+0·456		*+0·281*	*+0·343*	*+0·207*
Code	+0·244	+0·371	+0·538	+0 302		*+0·287*	*+0·125*
Consecutive digits	+0·361	+0·338	+0·188	+0·332	+0·276		*+0·320*
Rearranging digits	+0·349	+0·118	+0·216	+0·234	+0·158	+0·310	

These positive intercorrelations give more definite indication of a common factor for oscillation than did the corresponding table in the previous investigation.

VII. FACTORIAL ANALYSIS OF INTERCORRELATIONS

In an effort to confirm this suggestion the above table of raw correlations was subjected to multiple factor analysis as described by Thurstone ((4), appendix) and as exemplified by Alexander (1).

(1) The analysis showed that about 32% of the variance was accounted for by the first factor (see Table XII, p. 45 for loadings), and that of the residual correlations after it had been extracted, only one was as much as twice the P.E. of the original correlations. To express this in another way, when the analysis was continued to extract another factor, it accounted for only 8% of the variance, and a third factor for 7·4%.

Hence either from the point of view that the residual correlations after the first table are not significant (i.e. if the usual standards of significance hold for these residual correlations), or that any factors after the first account for a very small part of the total variance, it may be concluded that a single factor, which is not G, is sufficient to account for the observed intercorrelations.

(2) A similar result was obtained when the analysis was made with the coefficients which had been corrected for age. This time the first factor accounted for 30% of the variance (see Table XII, p. 45 for loadings), the second and third for 7 and 5% respectively. Also, only one residual correlation after the first table was as much as twice the original P.E. The elimination of age is seen to have increased the relative importance of the first factor. A more detailed account of further factor analysis will be given later.

VIII. ESTIMATION OF STEADINESS

To turn now to the other aspect of the investigation, the estimation of steadiness of character, various improvements in the methods of obtaining estimates were adopted, allowing of intercorrelations of known P.E. being obtainable.

The subjects comprized four school classes, and estimates of steadiness of character for each child were obtained from either three or four of its teachers, after at least one session's acquaintance. To assist the teachers in giving estimates of steadiness, a scale of nine points was suggested, and the number in each class was divided in a manner as nearly normal as possible among these nine positions. The teachers had then to grade the pupils relatively, putting the correct number in each position. They started from the least and most steady children, giving them estimates of 1 and 9 respectively, and so working in to the average child who would be given 5. This method overweights the extremes, but not to anything like the same extent as was previously found to be the case when the estimates were made without the guidance of a table of the relative numbers in each group. It has, moreover, the important advantages that the sets of estimates have all equal variances and the distribution of the mean estimates is not unduly narrowed ((6), p. 6, note).

In order to ensure that the estimates were being given as far as possible on the same grounds by the different teachers, it was suggested that the following points should be taken into consideration:

(1) Consider mainly consistency of conduct and behaviour, not merely excellence in classwork, e.g. a pupil may be as steadily poor as steadily good.

(2) Consider steadiness in (*a*) examinations, (*b*) classwork, (*c*) punctuality of person and of work, (*d*) any other aspect known to you.

The mean estimate of steadiness was obtained for each child from the three or four separate estimates, and the estimates of each teacher were correlated with these mean estimates, giving the coefficients shown in Table VIII.

Table VIII

	Teacher							
	A	B	C	D	E	F	G	H
Correlation with mean estimate	0·86	0·81	0·78	0·90	0·80	0·90	0·81	0·86
P.E.	0·018	0·033	0·040	0·027	0·045	0·024	0·056	0·036
Number in group	95	50	45	22	28	29	16	22

The smallness of some of the groups would usually prevent the legitimate use of the ordinary estimate of the significance of *r* from its P.E., but because of the almost normal distribution of estimates this objection does not apply so strongly in this case. The significance is probably exaggerated above, but is present nevertheless (2). The correlations are uniformly high, and show greater consistency than is obtainable from the tests given to the subjects. The greater correlation of some of the estimators than of others with the mean estimate is not due to the fact that the former have a larger standard deviation in their estimates, because the method used of giving the estimates ensured equal standard deviations. Also the coefficients are larger than would arise from a chance correlation between unconnected estimates and their mean.

IX. ESTIMATES AND OSCILLATION SCORES

The mean estimates of steadiness were correlated with the mean σ/M score for each test. All of the coefficients, as suspected from the previous investigation, were negative, and four significant. The figures are given in Table IX.

Table IX

	Test						
	1	2	3	4	5	6	7
r (mean score and mean estimate)	−0·12	−0·24	−0·19	−0·26	−0·30	−0·30	−0·09
P.E. of *r*	0·07	0·07	0·07	0·07	0·07	0·07	0·07

The fact that all the coefficients are negative strengthens the indication of an inverse relationship given by the four significant coefficients. A single figure showing the amount of connexion between the estimates and the tests was obtained by correlating the mean σ/M scores for all the tests with the mean estimates. This gave $r = -0{\cdot}320$ P.E. $= 0{\cdot}065$ which is significant. This figure is likely to be swollen by the combined effects of age and intelligence, and it is therefore desirable to see what effect the elimination of these two variables has on the coefficient. The correlation coefficients required to calculate the required partial correlation between tests and estimates are:

(Mean Score—I.Q.) $r = +0{\cdot}012$ (Mean Estimate—Age) $r = +0{\cdot}154$.
(Mean Estimate—I.Q.) $r = +0{\cdot}201$ (Age—I.Q.) $r = -0{\cdot}279$.[1]
(Mean Score—Age) $r = -0{\cdot}295$.

The resulting partial correlation is:

(Score-Estimate, Age and I.Q. eliminated) $r = -0{\cdot}275$,

which is still significant.

X. EFFECT OF MISTAKES

It was thought to be desirable to consider what connexion, if any, existed between the number of mistakes made by the subjects in the tests, and the other variables. In four of the tests, viz. code, rearranging digits, writing letters, writing numbers, mistakes were entirely absent, or negligible in number. In the other three they sometimes occurred to quite a large extent. Their number was seen to be roughly proportional to the speed of working, so a measure of the relative number of mistakes was obtained by using $\dfrac{\text{Number of mistakes}}{\text{Mean score}}$. This measure was correlated with the mean estimate, with the coefficient σ/M for oscillation and with I.Q., giving the coefficients shown in Table X.

Table X

	Correlation of mistakes with					
Test	Mean estimate	P.E.	σ/M	P.E.	I.Q.	P.E.
Consecutive digits	−0·164	0 07	+0·031	0·075	+0·114	0·08
Subtracting digits	+0·004	0 07	−0·101	0·07	−0·043	0·08
Adding digits	−0·099	0·08	+0·310	0·07	−0·220	0·09

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[1] This negative correlation usually occurs in the school from which the subjects were taken, when ages over 14 are included; i.e. the brighter children tend to (or tended to) leave before the others. Also each class included a few older, less intelligent children repeating a year.

Only one of these is significant, that for adding digits. This result is possibly due to the fact that adding digits was the first test to be given. This possibility is strengthened by the fact that only eight of the ninety subjects made any mistakes in the second application of this test. Hence these results cannot be taken to show the existence of any connexion between whatever is measured by mistakes and the other variables considered.

XI. TEST LOADINGS WITH THE GROUP FACTOR FOR OSCILLATION

To obtain more detailed information about the factor for oscillation common to the tests and estimates, and to discover especially its relationships with the tests, some further multiple factor analysis was undertaken. This time the intercorrelations with the tests for age, I.Q., and estimates were included, giving a ten-square matrix. From this matrix the analysis was continued until six factors, of diminishing importance, had been extracted. As previously found, the first factor was considerably more important than the others, but as was to be expected from the greater diversity of the entries in the matrix, it did not account for such a high proportion of the variance. The loadings of the matrix entries with the factors are shown in Table XI.

Table XI

	Factors							
	I	II	III	IV	V	VI	h^2	h
Estimate (mean)	−0·461	−0·192	−0·163	+0·512	+0·094	+0·014	0·547	0·740
Test: Adding	+0·582	−0·362	−0·086	+0·051	−0·215	−0·072	0·531	0·730
Consecutive digits	+0·515	−0·216	+0·427	−0·108	+0·057	−0·109	0·523	0·725
Code	+0·654	+0·294	−0·097	+0·062	+0·153	−0·099	0·560	0·749
Writing digits	+0·559	−0·055	+0·018	−0·041	+0·218	+0·014	0·366	0·606
Writing letters	+0·658	+0·137	−0·241	+0·258	+0·127	−0·197	0·633	0·797
Subtraction	+0·480	−0·005	+0·095	−0·103	+0·053	−0·128	0·269	0·520
Rearranging digits	+0·412	−0·286	+0·107	+0·137	−0·141	+0·185	0·337	0·581
I.Q.'s	−0·020	−0·539	−0·172	−0·100	+0·479	+0·392	0·714	0·846
Age	−0·328	+0·199	+0·624	+0·109	+0·117	−0·127	0·580	0·761
Σk^2	2·504	0·732	0·723	0·402	0·411	0·285	5·060	—
$\Sigma k^2/n$	0·2504	0·0732	0·0723	0·0402	0·0411	0·0285	0·506	—
%	25·04	7·32	7·23	4·02	4·11	2·85	50·6	—

These loadings do not give any indication that any one test, or the mean estimate, is identical with the first common factor, or with any of the others. Hence there was no advantage to be gained by rotating the axes to coincide with any particular test or tests. Instead the first factor loadings were compared with the correlations between the entries in the table and the mean σ/M for oscillation, giving Table XII. The last row of this table gives the loadings in an analysis of the seven tests and mental age, and shows that the effect of mental age is similar to that of chronological age, as was to be expected from the intercorrelations already given (Tables V and VI).

Table XII

	Test									
	1	2	3	4	5	6	7	Mean estimate	Age	I.Q.
Correlation with mean σ/M	+0·70	+0·60	+0·62	+0·52	+0·67	+0·61	+0·58	−0·32	−0·295	+0·01
First factor loadings from Table XI	+0·58	+0·48	+0·66	+0·56	+0·65	+0·52	+0·41	−0·46	−0·33	−0·02
First factor loadings from § VII (1)	+0·58	+0·49	+0·65	+0·58	+0·62	+0·55	+0·44	—	—	—
First factor loadings from § VII (2)	+0·55	+0·49	+0·61	+0·55	+0·59	+0·60	+0·41	—	—	—
First factor loadings (7 tests and mental age)	+0·60	+0·47	+0·67	+0·55	+0·67	+0·50	+0·42	—	Mental age −0·36	

The third and fourth rows give the corresponding loadings of each test with the first common factor from the analyses mentioned in § VII. It is seen from the above table that the loadings of the tests, from the four analyses, are in good agreement with one another, and that they, together with the loadings for age, I.Q. and estimates, are also in fair correspondence with the mean σ/M correlations. This correspondence gives further indication that the first, and most important, factor common to the tests is that of oscillation. To verify this point in yet another way, a further analysis was made, this time of a table containing the intercorrelations of the mean scores for oscillation (σ/M), the seven tests, and the mean estimates. The first common factor loadings and the correlations with the mean score for oscillation are shown in Table XIII.

Table XIII

	Mean score	Test							Mean estimate
		1	2	3	4	5	6	7	
Loadings of first common factor	+0·97	+0·62	+0·61	+0·645	+0·57	+0·63	+0·55	+0·48	−0·39
Correlation with mean σ/M	—	+0·70	+0·60	+0·62	+0·52	+0·67	+0·61	+0·58	−0·32

In this analysis the first common factor accounted for 39 % of the variance, and the next for 6 %. The loadings are nearer the correlations with the mean σ/M than in the other analyses, and the high loading of 0·97 of the mean score points again to the common factor present in the tests being that of amplitude of oscillation.

XII. SUMMARY

It has been shown that:

(1) A common factor other than *G* is present in the tests given and the estimates obtained for steadiness of character, and that this group factor is most closely identified with the mean score for amplitude of oscillation.

(2) This group factor is significantly and negatively correlated with estimates of steadiness of character.

(3) This group factor is independent of I.Q., and also is probably independent of what is measured by mistakes.

(4) For the age range of the subjects used (11½–16½ years) this group factor shows a significant decrease with age and also with mental age. Its presence is still evident when the effect of age has been eliminated from the intercorrelations.

In conclusion, the writer wishes to express his thanks to Dr R. H. Thouless for his assistance in the discussion and analysis of the results.

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CHANGES IN FIGURE-GROUND PERCEPTION IN PATIENTS WITH CORTICAL LESIONS

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I. *Introduction* (p. 47).
II. *Method of investigation* (pp. 47–48).
III. *Results* (pp. 48–50).
IV. *Discussion* (pp. 50–51).
References (p. 51).

I. INTRODUCTION

IN a previous investigation (3) an attempt was made to isolate the determining factors in figure-ground articulation using modifications of the Rubin profile-vase drawing (4). Perception of one part of the field as figure seemed to depend, with normal subjects, on its smallness, surroundedness, the good continuation and closure of its lines, its greater articulation. It was also found that a truly ambiguous figure, i.e. one in which none of these factors was operating, would be seen in terms of a previous perception in which one part of the field had definite figural properties; but this influence from a previous perception, established attitude or 'carry-over' from one figure to another, did not occur when the second drawing possessed other and opposite figural properties. In other words, if an 'ambiguous' figure followed one in which the profiles had been enhanced the tendency was for it to be seen as profiles also, but even a slight enhancement of the vase was sufficient to destroy any effect of the previous perception.

II. METHOD OF INVESTIGATION

The present investigation deals with the question of figure-ground perception in patients with cerebral lesions. The performance of 30 patients with cerebral lesions (the experimental group) was compared with 30 'normal' patients, i.e. patients without brain involvement, from the same hospitals. The experiments were carried out at the Montefiore Hospital in New York, and the Neurological Institute in Montreal.[1]

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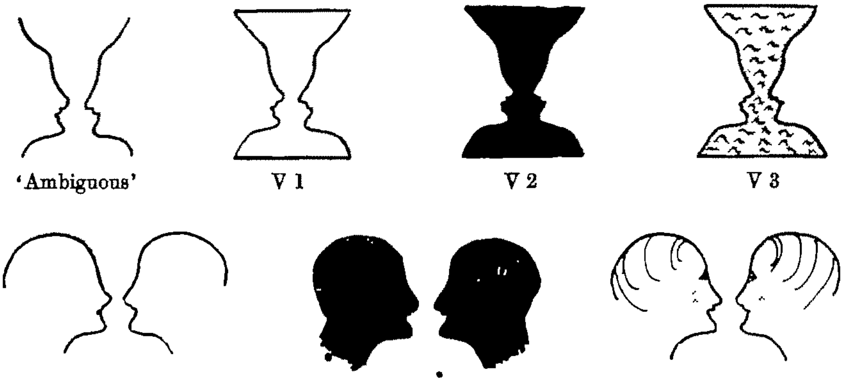
[1] The author wishes to express thanks to Dr K. Goldstein for his interest and encouragement in this study; and to Drs W. Penfield and W. V. Cone for the co-operation which made it possible.

No differentiation on the basis of localization of the lesion was made in this investigation. The patients were in the main post-operative cases with unilateral lesions, both hemispheres, frontal, frontal parietal, and temporal lobe cases being represented.

Fig. 1[1] shows the modifications of the original drawing which were used. These consisted of a series of seven in which three enhanced the profiles, three the vase, while one was an ambiguous figure in which profile or vase could be seen equally easily by normal subjects. These drawings, on large cards, were presented singly to the patients. The order of presentation was either:


'Ambiguous' V 1 V 2 V 3

P 1 P 2 P 3

Fig. 1.

Ambiguous, Profile 1, P 2, P 3, Ambiguous, Vase 1, V 2, V 3, Ambiguous; or, Ambiguous, Vase 1, V 2, V 3, Ambiguous, Profile 1, P 2, P 3, Ambiguous. When the first Ambiguous figure was seen as a Vase, the subsequent card was Profile 1, and vice versa.

The instructions were simply that the patient should tell what he saw.

III. RESULTS

A normal record was considered to be one in which the ambiguous figure was seen either as profile or vase, the subsequent drawings in terms of their figural enhancements, the repeated ambiguous figures either in terms of how they were first seen, or modified by an attitude created by the preceding drawings. Twenty-seven, or 90%, of our normal patients gave such records; only two patients, or 6·6%, in the experimental group gave 'normal' records.

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[1] V 3 was red in the original, with black markings; P 3 had red cheeks, and blue eye markings.

This leaves the twenty-eight records in the experimental group (and the three who deviated in the normal group) to be considered to see what light they can throw on figure-ground perception in cases of cerebral lesions.

The deviations from the normal record were of four kinds:

I. There is a marked perseveration of the attitude engendered by the first perception, such that the figure seen in the first perception is maintained despite counter influences of figural properties.

Example. If the first ambiguous figure is seen as profiles, no vase will be seen in the subsequent drawings despite their 'vase enhancement'.

II. There is a failure to recognize the ambiguous figure as any meaningful object, even after correctly perceiving all the other figures.

III. There is a rigidity such that the less dominant figure cannot be seen at will, despite its having been perceived in other drawings in which it was the dominant figure.

IV. There is an introduction of other objects in the place of profiles or vase, either throughout the whole series or in parts of it.

Example. One subject in the experimental group saw such a variety of objects as 'a mound of hay' (Amb.) (P. 1); 'Priests' (P. 2); 'People' (P. 3); 'a table' (V. 1); 'a priest's hat' (V. 2); 'a cardinal's hat' (V. 3).

Table I gives the distribution of these deviations among the experimental and control groups.

Table I

	Cortical group	Controls
Perseveration:		
3 places	4	0
2 places	4	0
1 place	2	2
Total	10	2
Failure:		
On ambiguous figure	8	1
On other figures	4	0
On all except P. 3	1	0
Total	13	1
Introduction of new objects	6	0
Inability to shift	4	0
Normal records	2	27
Total no. of deviations*	33	3

* Since some records deviated from the normal in more than one way, the total of such deviations in the cortical group is 33; i.e. 28 patients out of the 30 showed one or more deviations.

Table II shows examples of typical deviations as they appear on the records.

Table II

Order of presentation	Perseveration				New objects	New objects	Failures		
Amb.	P	P	P			Fly	–		–
V. 1	P	P	P			Mushroom	V		–
V. 2	P	P	V			Tulip	V		–
V. 3	P	–*	V			V	V		–
Amb.	P	P	P	V	Hay	–	–	–	–
P. 1	P	P	P	V	Hay	Octopus	P	P	–
P. 2	P	P	P	P	Priests	P	P	P	–
P. 3	P	P	P	V	People	P	P	P	P
Amb.	P	P	P	V	Hay	–	–	–	
V. 1				V	Table			V	
V. 2				V	Priest's hat			V	
V. 3				V	Cardinal's hat			V	
Amb.				V	Hay			–	

* – stands for failure to perceive any object.

Two other characteristics of the responses of the cerebral group may be mentioned here. Whereas the normal subjects frequently mentioned casually that these drawings were such that both figures could be seen in quick succession, even if only one 'hit you in the eye' to start with, only one patient in the cerebral group ever made such a comment, and in this case it was for him a genuine discovery which caused him considerable excitement. This does not mean that these patients failed to have both experiences, but the dual aspect, the ambiguity, the variation on a central theme, seems to have been less striking to them. The other characteristic of the records of the cerebral group was the more frequent reference to concrete details in the pictures; the 'man and woman', Roosevelt, Washington, the red cheeks and black hair of Profile 3, etc., were commented on.

IV. DISCUSSION

Brief mention must be made of the theoretical implications of these results. We have found that in these patients a simple perceptual experience has different properties from those found in normal subjects. As many as 43% are unable to make anything out of the ambiguous figure, a drawing, however, which presents no puzzle for normals. One third of the patients show the influence of an established attitude, a tenacious holding to the impression created at first, in the face of a counteracting perceptual influence, and an inability to pick out the two aspects of the figure simultaneously. We also find the rather surprising introduction of other, and not necessarily relevant objects, and the lack of ability to see the series as a series. But what do these things mean?

We must assume, I think, from the failure on the ambiguous figure that spontaneous sensory organization differs in such patients in that a given retinal stimulus distribution, which would produce a well organized experience in normals, may result in chaos, failure, for some of these patients.

Secondly, the introduction of familiar objects, as for example the mound of hay and the priests seen by the catholic farmer, the lack of ability to see the series in its serial character, the emphasis on concrete detail, all point to the fact that these patients are less capable of taking a detached attitude in regard to their perceptions, and, where possible, incorporate them in a framework of tangible and concrete reality... evidencing perhaps what Goldstein (1,2) has called the 'loss of abstract attitude'.

Finally the marked perseveration of the original perception, the clinging to the established attitude, seems to be also a matter of this lack of detachment... the Ego, the core of the individual one might say, is involved to a greater degree even in such simple performances, and 'failure' becomes a dangerous situation to be avoided, while the perpetuating of a successful response becomes a safeguard against possible difficulties. This appears most strikingly in certain individual patients who show real distress on being unable to make anything out of the ambiguous figure. Again, those patients who only see one figure throughout frequently assert that response with undue vehemence.

The value of such findings would seem to lie in the fact that they may give us clues to certain qualitative differences in the experiences and behaviour of persons suffering from cerebral lesions, differences which might well leave unaltered a quantitative estimate of the intelligence level, and might well pass unnoticed in ordinary daily life, and yet if recognized may enable us to reconstruct more accurately the changed psychological world resulting from changes in the brain.

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BINAURAL LOCALIZATION WITH TWO NOTES DIFFERING IN PHASE BY 180°

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I. *Preliminary experiments* (pp. 52–53).
II. *Results of main experiments* (pp. 53–56).
III. *Previous work* (p. 56).
IV. *Summary* (p. 56).

I. PRELIMINARY EXPERIMENTS

It is well known that if two notes of the same pitch are presented to the two ears, and it is arranged that they are of equal loudness and are in phase with one another, the sound which is heard appears to come from a source located somewhere in the median plane, though different observers vary as to its position in that plane. During some experiments in which the phase of one of these notes was changed by 180°, it was found that the sensation then produced by the two notes was quite different from that in the first case. The observer was conscious of a stimulus applied to each ear; the sensation was symmetrical but the source was no longer localized sharply in the median plane. When a sudden change from zero phase difference to 180° phase difference was made, a broad, diffused source of sound seemed to have been substituted for the apparent point source of the first case. This change was easily detected by the observer. The two telephones employed were fed from two separate secondaries on the output transformer of the amplifier following the heterodyne oscillator from which the pure tones used were obtained, and the connexions to one of these secondaries could be reversed by a switch. Interruptions of the telephone currents were made for about 1 sec. every 5 sec. During any one of these intervals of silence, a reversal could be made unknown to the observer; such changes were very rarely missed. The observer sat in a soundproof room and signalled when he considered that a change had been made. The frequencies used were 200, 400, and 3000 c./sec., and tests were made on three observers.

An investigation of the change of intensity required in one ear to produce a definite localization to one side or the other was next made for both cases, in the expectation that one might prove more sensitive to such changes than the other.

Two amplifiers supplied the telephones, which were each in series with a 1-megohm resistance. One of these circuits was connected to a centre-tapped transformer, with the centre-tap earthed, and a switch for connexion to one or other of the outer terminals for phase-changing; the terminal not in use was earthed through another 1-megohm resistance. The voltages applied to the output circuits were arranged to be in phase by using a cathode-ray oscillograph in conjunction with a variable condenser (0·0015 μF. max.) across one of the circuits. Since the output circuits are of high resistance and low inductance, the currents in them, and therefore in the telephones, will differ very little in phase.

In the first results taken, the intensities used were sufficient to give a moderately loud note, say 30–40 db. above threshold. After the phase correction had been made and the intensities approximately equalized by obtaining a straight line at 45° to the axes on the oscillograph, small intensity adjustments were made to obtain 'central' localization. Then a series of readings was taken; the intensity was reduced slowly in each ear in turn, and the observer signalled when he detected a change in the localization of the combined note. The oscillograph showed that there was no phase change with the intensity changes actually needed. In every case the observer noticed the 'return swing' of the combined note when the original symmetrical conditions were restored. Results were taken at 400 and 800 c./sec. in this way with two observers.

In the ordinary case of zero phase-difference, change of intensity in one ear sufficient to disturb the central localization causes an apparent angular shift of the sound out of the median plane. In the present case no such steady angular shift occurred; the change was from symmetry to a note located definitely in one ear.

II. RESULTS OF MAIN EXPERIMENTS

A further series of results was then taken with known intensities of the notes presented to the ears. Shunts were used on the telephones, and were adjusted to give monaural threshold intensity at 0 db. on the attenuator in each amplifier. Then settings were made as before with the attenuator at +50, +40, +30 and +20 db. It was found that it was better to make the intensity adjustment before starting when the phases differed by 180°. Ten settings were made for each result in Table I, and the same two frequencies were employed. Most of the results were taken by two observers; one of them (J. W. H.) required far greater changes than the other (W. L. B.) though the 'return-swing' was easily detected by the latter. In fact, he complained that the rate of decrease of intensity was too rapid in the first trials, and it was reduced when taking the results shown. Some settings by a third observer (J. H. S.) agreed roughly with those of the second.

Table I. *Gradual intensity change (db.) for just perceptible change of location*

Observer ...	J. W. H.				W. L. B.				J. H. S.			
Frequency ... (c./sec.)	400		800		400		800		400		800	
	0°	180°	0°	180°	0°	180°	0°	180°	0°	180°	0°	180°
Intensity												
+50 db.	—	—	4·3	3·1	—	—	—	—	—	—	—	—
	2·0	2·5	4·8	2·6	1·1	0·9	0·8	1·0	0 5	0·4	2·4	1·4
+40 db.	5·8	2·6	5·4	3·0	1·1	0·8	1·1	1·1	—	—	—	—
+30 db.	7·5	4·5	6·7	3·9	1·1	1·0	0·9	1·0	—	—	—	—
+20 db.	7·8	5·3	11·8	3·9	1·0	1·3	1·0	0·7	0·7	0·7	1·4	1·7
+30–40 db.	—	—	2·2	1·1	—	—	0·7	0·5	—	—	—	—
	5·8	4·5	2·1	1·2	1·2	1·2	0·8	0·7	—	—	—	—

To find whether this discrepancy was due to some difference between the observers' uniaural differential thresholds, these were determined for each observer at the frequencies and intensities employed in the foregoing. The output of one amplifier was used, and a fixed shunt of 1000 ω, with a variable resistance R in series with it, was used on the telephone. It was arranged that R was short-circuited for about half a second every second, so that the intensity in the telephone rose and fell twice a second. The note was presented to the observer at the required intensity, and the value of R was reduced or increased in accordance with his signals to determine the value of the uniaural differential threshold. Then this threshold is given approximately in decibels by $20 \log_{10} (1+R/1000)$. Five settings were made at each intensity; if a rest were required, the observer could silence the telephone between settings by a switch. It will be seen from Table II that there is little difference between the results for the three observers.

Table II. *Uniaural differential thresholds (db.)*

Observer ...	J.W.H.		W.L.B.		J.H.S.	
Frequency (c./sec.) ...	400	800	400	800	400	800
Intensity						
+50 db.	0·7	0·6	0·6	0·4	0·4	0·6
+40 db.	0·9	0·7	0·7	0·4	—	—
+30 db.	1·0	0·5	0·7	0·4	—	—
+20 db.	0·9	0·7	0·7	0 4	0·4	0·5

Using this latter form of the apparatus, the detection of changes of location caused by sudden change in intensity was studied. The notes were balanced in intensity as in the first experiments, and then R was increased from zero to about 100 ω. The location of the note then changed from the median plane to one side when the phase difference was zero, or from the broad and diffused type of source to a note definitely in one ear when the phase difference was 180°. As R was reduced according to the observer's requirements, this change gradually disappeared, and the value of R when it could no longer be detected was found. The variations in intensity caused by R were still noticeable at this stage. In these experiments it was found advisable to double the times for which R was open- and short-circuited. The results for two observers at various intensity levels and two different frequencies are given in Table III. The intensity changes needed by the second observer (W. L. B.) are again lower than those required by the first (J. W. H.), and though they are of the same order as his differential thresholds, he (W. L. B.) maintained that intensity variations were still audible at the settings made.

Table III. *Sudden intensity change (db.) for just perceptible change of location*

Observer ...	J. W. H.				W. L. B.			
Frequency (c./sec.) ...	400		800		800		400	
	0°	180°	0°	180°	0°	180°	0°	180°
Intensity								
+50 db.	4·0	2·5	2·3	2·3	0·6	0·5	—	—
+40 db.	2·8	2·0	2·3	2·8	0·6	0·6	0·6	0·6
+30 db.	3·2	4·3	2·9	2·3	0·7	0·6	1·1	0·9
+20 db.	3·5	2·9	2·6	3·2	1·0	0·8	0·9	0·8

A comparison between the just perceptible gradual changes of phase for differences of phase between the notes of 0° and 180° was also made. These tests were carried out during some work on the variation of the amount of these changes with frequency; it is hoped that a fuller account of the apparatus and the methods used will be published shortly. Both phase and intensity of either note could be independently varied. The phases of the notes were equalized (or opposed) before the start of an experiment, and the intensity of one varied a little to give 'central' localization as already described. At a given signal, the phase of one note was slowly changed until the observer signalled that a change of location had been detected. Precautions were taken to ensure that no external criteria were used. All the measurements were taken with notes at 50 db. above threshold.

The results for the three observers indicate that there is no significant difference between the least perceptible changes of phase detectable with either phase relation at a given frequency. Both show an increase in the amount of change required as the frequency is increased, and at about

1500 c./sec. it becomes impossible to detect a change of phase with either phase relation. A typical set of figures is shown in Table IV; they seem to indicate that failure occurs at a rather lower frequency when the notes differ in phase by 180°. These deductions are confirmed by some further experiments in which sudden phase changes were made and the least perceptible change was found.

Table IV. *Gradual phase change (degrees) for just perceptible change of location (J. W. H.)*

Frequency (c./sec.)	...	600	700	800	900	1000	1100	1200	1300	1400
Phase difference	0°	24·5	28·0	23·3	23·7	20·8	22·2	31·0	59·0	—
	180°	30·0	20·0	28·3	27·5	21·6	30·0	108·0	113·0	—

III. PREVIOUS WORK

It is of interest to note that in the experiments of Halverson[1] on binaural localization the effect of varying the relative phases of the two notes was investigated, and the subjective phenomena when the difference was 180° were also noted. His observers stated that the sensation was one of a double sound of equal intensity at the two ears, and that there was no clear image in the median plane. As the phase was varied slowly through the position of 180° phase difference, the combined sound did not pass through the head; the observer was conscious of two distinct images on the aural axis, and as the phase changed one approached the median plane from one side, while the other receded and finally disappeared. Though the observers in the present work were unaware of these experiments and made no special introspections as to the nature of the change, the descriptions of the effect offered by them and given above are much the same as those of the trained introspective observers of Halverson.

IV. SUMMARY

The least perceptible variations in the relative phases and intensities of two notes differing in phase by 180° and 0° have been compared, and little difference found between them. The sensations experienced in the first case are described.

The author wishes to thank the Medical Research Council for grants which made the work possible, and to express his appreciation of the suggestions made by Dr J. H. Shaxby, in whose laboratory the experiments were performed.

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[1] Halverson, H. M. (1922). *Amer. J. Psychol.* XXXIII, 178.

(*Manuscript received* 3 *August* 1938)

SOME RACIAL DIFFERENCES IN PERCEPTION

By W. M. BEVERIDGE

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I. PHENOMENAL REGRESSION FOR BRIGHTNESS AND WHITENESS

(1) *Method of experiment*

In a former paper (1) it was shown that West African students show a greater tendency to phenomenal regression for shape and size than do British students. The aim of the following experiments was to find out if these African students showed also a greater tendency to phenomenal regression in the perception of brightness and whiteness.

As in the earlier tests the subjects were natives of the Gold Goast, Ashanti and Togoland, male students of the Presbyterian Training College, Akropong; on the average they were about eighteen years of age. The procedure was a copy of that used by Dr Thouless in his experiments with Scottish students (2). A white disk 4 in. square was supported at right angles to the table and to the subject's line of vision, at a distance of 290 cm. from the source of light. A grey disk of the same size was arranged so that the subject saw it to one side of the white disk and separated from it. The distance of the grey disk from the lamp was adjusted until the subject reported that the two disks looked equal in brightness or whiteness as the case might be.

The disks were cut from Zimmermann's series of fifty grey papers, one being the standard white and the other no. 13 grey. The latter had a reflectivity 0·228 that of the white. The sole source of light was a 4½ v. electric bulb, and the subject sat behind the lamp.

The instructions were: "That is a white disk, and this is a grey one. When I hold it here (holding the grey disk well behind the point at which its luminosity equalled that of the other) it looks less bright than the other. As I bring it nearer to the lamp, it looks brighter and brighter, until finally it looks brighter than the other one. Now I want to find the exact position at which it looks the same brightness as the other one."

After preliminary practice three ascents and three descents were made, and the mean was taken to represent the position at which the disks seemed equally bright for that subject. The same procedure was used to determine the position of equal whiteness. On each ascent and descent the subject balanced both for brightness and for whiteness. As in the earlier tests the measure used was the 'Index of phenomenal regression' (P.R.), i.e. $(\log P - \log S)/(\log R - \log S)$, where P is the numerical value of the phenomenal character, S that of the stimulus character, and R that of the real character.

(2) *Results*

Forty subjects were tested. None of them failed to make a considerable difference between the position for brightness and that for whiteness. Only in one case was the ratio of P.R. for whiteness to that for brightness less than 1·35. This makes an interesting contrast to Dr Thouless's results(2); out of forty-five Scottish students he found twenty-two for whom that ratio did not exceed 1·35, and for ten of these the position of equal phenomenal brightness was also that of equal phenomenal whiteness.

The mean African P.R. for brightness was found to be 0·281. The European mean found by Dr Thouless was 0·325. The difference is 0·044, and the P.E. of difference 0·015. There was only one negative case. Thus, contrary to expectation, the African students showed *less* P.R. for brightness than did the European students. The medians and quartiles are shown in Table I.

Table I

Group	No.	Lowest	First quartile	Median	Last quartile	Highest
		Index of P.R. for brightness				
European	104	(Negative)	0·19	0·305	0·47	0·66
African	40	(Negative)	0·15	0·28	0·39	0·56
		Index of P.R. for whiteness				
African	40	0·548	0·666	0·708	0·729	1·00

The mean African P.R. for whiteness was 0·698. One subject saw the grey paper still grey even when it was touching the lamp. Dr Thouless publishes a list of only eight subjects' P.R. for whiteness; their mean is 0·606. This is 0·092 less than the African. As the sample is so small we may apply the χ^2 test, forming a fourfold table about the median of the group as a whole(3). With Yates's correction for continuity we find χ^2 equal to 3·75. The likelihood of this occurring by chance sampling is practically one in twenty.

Thus we find that while the African students have less P.R. for brightness than the Scottish students, they have probably more for whiteness. It was whiteness, not brightness, however, which Dr Thouless found to correlate highly with phenomenal regression for size and shape. It is interesting to note that with the Africans there seems to be a small *negative* correlation (−0·27 ± 0·098) between P.R. for brightness and P.R. for whiteness.

II. A PICTURE PREFERENCE TEST

The aim of this experiment was to discover if the African's greater tendency to phenomenal regression affected his appreciation of pictorial art. Dr Thouless has shown that certain features of Oriental art, "the absence of shadows, and the partial or total absence of perspective", are probably due to the fact that Orientals have a large tendency to phenomenal regression(4). These features are seen also in the work of African artists such as Enwonwu, Ibrahim, Nnachy, and Ibeto(5, 6), and in the drawings of African school children and college students. Do Africans, therefore, regard these features as merits? Do they prefer Oriental pictures to European?

The subjects were again Gold Coast students. The material used was a set of eighty coloured postcards, forty of famous European pictures, and forty of Japanese, Indian, and Persian pictures. (The former were mostly published by The Medici Society, and the latter by the trustees of The British Museum.) These pictures were paired roughly according to subject, and exhibited on a board, eight at a time, the European picture being sometimes on the right hand and sometimes on the left of the Oriental one, e.g.

Sheltering from a Shower (Kiyonaga)	The Little Street in Delft (Vermeer)
From the May Border (Whittington)	Bird on a Rose-tree Branch (Hokusai)
Child Upsetting a Goldfish Bowl (Utamaro)	Pas Mêche (Bastien-Lepage)
Head of a Girl (Vermeer)	Two Girls (Shigemasa)

The subjects were unaware of the fact that one picture in each pair was European and the other Oriental.

Each student was asked to state on a slip of paper which picture in each pair he preferred and also which one of the eight he liked best of all. About a hundred men took part, but not all voted on every set of pictures; 2827 votes were cast.

The results were contrary to expectation. Western pictures received 2480 votes (86·5% of the total number), while Oriental pictures received only 337 votes (13·5%). Only two Oriental pictures were more popular than the corresponding European ones; "Ten Thousand Acres" by Hiroshigi was preferred to Farquharson's "Dawn", and the "Persian Miniature from the Fable of Kalilah wa Dimnah" was more popular than Margaret Tarrant's "Sylvan Melody". In no case was an Oriental picture the favourite on the board.

These results show clearly that, in spite of high phenomenal regression, African students prefer European to Oriental art. We must remember, however, that these subjects had been used to seeing European pictures from their earliest school-days, whereas Oriental art was new to them. Had they been untouched by European influences the results might have been quite different.

It is interesting to note some of the reasons given for liking pictures. Of these the most important seemed to be the illusion of reality: "Anybody looking at the picture thinks that parts of the paper are bulging out"; "The work does not look flat", "The picture represents things naturally so as to make people forget themselves when they are looking at it." A glance at the most popular picture of each set confirms this: "From the May Border" (Whittington), "The Angelus" (Millet), "La Laitière" (Greuze), "Flatford Mill" (Constable), "The Cornfield" (Constable), "The Boyhood of Raleigh" (Millais), "Dutch Interior with Soldiers" (de Hooch), "Opal Morning" (Mary Holden Bird), "The Duchess of Milan" (Holbein), "The Young Warrior" (Rembrandt).

Admiration for the artist's technique was also frequently expressed: "The quality and folds of the cloth are very well shown"; "The colours merge very gradually"; "The light and shade are very skilfully painted"; "One can see the pattern on the lady's dress and the dazzling earring". Some pictures were liked because they told a story, and others because they seemed to represent ideal conditions: "Everything in the room is neat and in its proper place"; "The lady's hair is beautifully brushed and combed"; "The building is a masterpiece of modern architecture; it is like the palace of a great monarch". This last was a reference to Vermeer's "Little Street in Delft"!

III. MENTAL COMPROMISE IN ESTIMATING DIRECTION

(1) *Method of experiment*

The following test was devised to find how far one's judgment of the horizontal depends on visual clues; to study individual and racial differences in this; and to discover if it is connected in any way with age, intelligence and temperament.

The apparatus used was a large wooden cupboard, 6 ft. 6 in. by 54 by 20 in. It was mounted so that it could be easily tilted to an angle of 25°. A stick, 29½ by ¾ in. by ¾ in., was hinged to the end of the cupboard 29 in. from the floor so that it could swing up and down; this was connected with a pointer which moved across a dial on the exterior of the cupboard. There was a small translucent window 9 in. square in the roof. The interior was dark, unpainted wood.

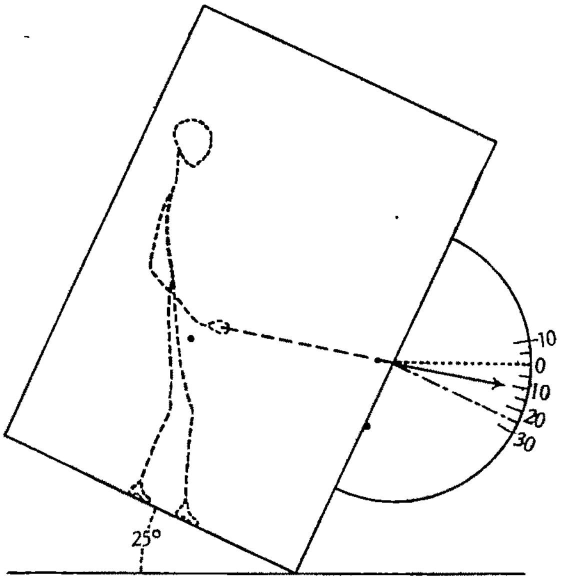

Fig. 1. On the indicator ········ shows the horizontal and —·—·—·—· the direction of the floor of the cupboard.

The subject entered the cupboard when it was upright, was shown the stick and its movement, and given the following instructions:

> When the door is shut the cupboard will be slowly tilted; hold on to the sides while this is being done; then take the stick and hold it at what *seems* to you to be level with the ground outside, so that if a dish full of water were sitting on the stick, the water would not run out. Do not try to calculate, but hold it at what *looks* level to you.

After two or three preliminary experiments the subject raised the stick and brought it down to apparent level, then lowered it and brought it up to apparent level. Five ascents and five descents were made in this way, and the mean was taken to represent the phenomenal level for that subject.

The subjects were fifty-two West African male students, on the average about 19 years of age, and twenty Europeans, ten men and ten women, ranging in age from 25 to 50 years, the average being about 35.

(2) *Results*

With six exceptions (two Europeans and four Africans) there was a large mental compromise; the visual clues afforded by the walls, floor and ceiling of the cupboard greatly affected the judgement. As a simple measure of this let us take the error of the subject's estimation divided by the angle of tilt of the cupboard, 25°. It is obvious that if a subject were guided entirely by the visual clues he would hold the stick at an angle of 25° off level, and his score would be 1·00. If he were unaffected by visual clues he would hold the stick level and his score would be 0·00. Apart from the six exceptions mentioned above, whose scores were negative, all the scores lay between these extremes. The European mean was 0·498 (12·45° of error); the African mean was 0·418 (10·45° of error). There were great individual differences, the results lying roughly on the normal curve. The medians and quartiles are shown in Table II.

Table II

Group	No.	Lowest	First quartile	Median	Last quartile	Highest
African	52	(Negative)	0·219	0·433	0·619	0·900
European	20	(Negative)	0·434	0·543	0·642	0·858

The subjects whose scores were negative present an interesting problem. They may be compared with those whose scores were negative in the 'Brightness' test for phenomenal regression(2). In this present case, however, there must presumably be some kind of overdone reaction against what is felt to be the misleading nature of the visual stimuli.

In most cases the subject's judgement in this experiment was not so stable as in those for phenomenal regression. Complicating factors such as mental set, and possibly fatigue and health, may affect the judgement. In actually performing the experiment one has a feeling of being slightly dazed or bewildered, probably because sets of perceptual cues, usually found to be in agreement, are here in conflict.

Considering these facts we must be cautious in drawing conclusions from this test. As Table II shows, however, there seems to be a slight racial difference, the Africans on the average estimating level more accurately than the Europeans. This was to be expected; their greater tendency to phenomenal regression suggests that their perception is guided less by visual and more by other cues than is that of the European.

(In teaching and in giving intelligence tests to Africans I have found them apparently less interested in and efficient with pictures and diagrams than most Europeans, and proportionately more so with words.)

To test the significance of this apparent racial difference we may form a fourfold table round the median of the two groups taken as a whole (see Table III).

Table III

	No. below median	No. above median
Africans	30	22
Europeans	6	14

$\chi^2 = 4{\cdot}43$

A value of χ^2 is thus obtained which would not occur by the chances of sampling from identical populations more than once in about twenty-seven times. This apparent racial difference may be due in part to the European subjects being older; but, on the other hand, they were almost certainly also more intelligent, and these two factors would tend to cancel each other out. We must, however, remember the probable effect of practice; from early childhood Africans are used to carrying loads balanced on their heads.

(3) *Relationship of mental compromise in the estimation of direction with other factors*

(*a*) *Age.* The African subjects varied little in age. With the Europeans there seemed to be a tendency for age to increase the ratio of mental compromise. The correlation was 0·517 ± 0·112. From Fisher's Table VA (3) we find a value of P practically equal to 0·02. Taking into consideration the similar connexion between age and phenomenal regression, we have the suggestion that the effect of experience on perception increases with age at the expense of the effect of the stimuli actually present.

(*b*) *Intelligence.* On the basis of personal knowledge and their College record, 24 of the Africans were ranged in order of intelligence. A correlation of −0·41 was found between intelligence and ratio of mental compromise in estimating direction. This is just significant ($P = 0{\cdot}05$).

(*c*) *Temperament.* Three people working independently arranged the twenty Europeans into five classes ranging from very extroverted to very introverted. As the correlation between the three gradings was high, the combined grading may be regarded as fairly accurate. There seems to be a connexion between extrovert tendencies and mental compromise in direction. The correlation was 0·486 ± 0·114. From Fisher's Table VA we find that P is roughly 0·03. As a further test, fourteen Africans were chosen by two observers as being extroverted and fourteen as being introverted. A fourfold table was formed giving a value of $\chi^2 = 2{\cdot}33$. This would occur in chance sampling about once in seven times, but it tends to confirm the previous result.

(*d*) *Phenomenal regression.* There seems to be a positive connexion between this and tendency to mental compromise in direction. Unfortunately only twenty subjects did both tests, but a value of P was found equal to 0·036.

IV. SUMMARY

1. African students show less phenomenal regression for brightness, more for whiteness, and therefore more discrimination between the two, than do British students.
2. This great tendency to phenomenal regression is reflected in African art, but it does not lead African students to prefer Oriental to European pictures.
3. Our perception of the horizontal depends to a considerable extent upon visual clues. When these contradict the more reliable internal clues, most people make an unconscious mental compromise.
4. There are very large individual differences in the amount of this mental compromise.
5. There is very probably also a racial difference, Africans being less affected by the visual clues than Europeans.
6. Mental compromise in estimating direction has a positive correlation with age, with extrovert tendencies of temperament, and very probably with phenomenal regression. It probably has a negative one with intelligence. Further investigation is, however, necessary.

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(*Manuscript received* 28 *October* 1938)

PUBLICATIONS RECENTLY RECEIVED

General Psychology from the Personalistic Standpoint. By WILLIAM STERN. Translated by H. D. SPOERL. New York: Macmillan. 1938. Pp. xxii+589. 20*s* net.

Titchener has often been held out to us as the Englishman who represented the German psychological tradition in America, but it is seldom recognized that the late William Stern has a strong claim to the title of the Jew who represented the British psychological tradition in Germany. If only for this reason, Dr Spoerl's distinguished translation of the *Allgemeine Psychologie* is worthy of a warm welcome in this country. It is quite impossible to read this book and to remain unimpressed by the remarkable parallelism between Stern's systematic outlook and the central tradition of British psychology. Stern's *Personalistik*, in its psychological aspects, is essentially the position of Ward restated in the context of contemporary experimental science. But this psychology, unlike Ward's, is no 'generalized' psychology. Stern's early training in Applied Psychology has left him in no doubt as to the supreme reality and significance of individual differences. The roots of 'personalistics' clearly go back to the classical researches embodied in the *Differentielle Psychologie* and the celebrated series of *Beitrage zur Psychologie der Aussage*. And these, in their turn, may surely be traced to the epoch-making contribution of Galton.

William Stern's theory of memory, elaborated at great length in Part III of this book, is of particular interest to English readers. In the first place, he rejects out of hand the outworn doctrine of memory traces, and introduces in its stead a theory of psychical dispositions almost identical with that of Stout. In the second place, he accepts as fundamental the distinction between 'bound' and 'free' ideas—a distinction which most of us have come to associate so intimately with the writings of Ward, Stout, McDougall and Hobhouse. And in the third place, his treatment of remembering itself, with its emphasis upon fluidity and construction, challenges serious comparison with the more recent writings of Bartlett. The insufficiency of associationism, moreover, was clearly appreciated by Stern, and the necessity for a dynamic approach to the central problems of psychology is exploited with a persuasiveness reminiscent of Stout and a vigour unmatched in the most outspoken polemics of McDougall. William Stern is surely entitled to a high place in a generation of psychologists whose principal contribution has lain in freeing their science from the arid sterility of the Leipzig tradition.

Dr Spoerl deserves our warmest congratulations upon the competence and skill with which he has carried out a difficult piece of translation. The text of the original German edition (which appeared in 1935) has been considerably revised, the author tells us in his Preface, "to meet the requirements of English and American readers". The bibliography, too, has been extensively modified, presumably with the same somewhat nebulous end in view. This appears in six parts, and is comprehensive enough, though much of the material quoted has little explicit reference to the text which precedes it.

It is to be hoped that the publishers will see their way to preparing an English edition of this book, more dignified in appearance and less costly in price. Such a step would undoubtedly assist in realizing the author's wish that "the English form of the book may strike a sympathetic note".

Human Understanding and its World. By K. W. MONSARRAT. University Press of Liverpool (London: Hodder and Stoughton). 1937. Pp. xx+480. 15*s.* net.

Human Powers and their Relations. By K. W. MONSARRAT. University Press of Liverpool (London: Hodder and Stoughton). 1938. Pp. xvi+289. 10*s.* 6*d.* net.

These two books have to be considered together, and represent a work planned and carried out on the grand scale. Together they range over the greater part of human history and they cover a vast number of specialized problems in both physical and biological science. In a very wide sense indeed they may be considered as a contribution to psychology, for they deal with human ideas and with the relation of these to the social and material world. But the empirical psychologist looking for indications as to exactly how the ideas which are discussed at length are formed, or even precisely what they are to be taken to mean, will be somewhat disappointed.

The sub-title of the first volume is *A Study of Societies.* There are three parts named respectively *Illustration of Imagination in History, The Scientific Image,* and *The Making of the Image and its Significance.* Image and imagination here are general words for any process of contact with and interpretation of the external world. The general contention is that any human individual knows himself and his fellows in two ways: directly, by way of action, and indirectly through the medium of some sensorial equipment. From this he tends to look upon himself and the world as constituted by a duality of 'power' and 'matter'. But this is a mistake, because the second interpretation is really derivable from the first, and as soon as the relativity of all indirect ways of 'knowing' is recognized, it becomes clear that all nature must be regarded as 'purely dynamic'. Not only so, but, apparently, we have to admit that all world 'assemblies' are made up of 'power units' of the same order though of very varying behaviour characteristics. That units of this kind should join into dynamic systems is a 'common terrestrial event', and wherever such systems are found it is also found that apparent behaviour is the expression of a 'universal inclination to states of balance' and a tendency to seek more and more stable and wide integrations.

The second volume is occupied by a prolonged discussion of the ways in which the author believes these hypotheses to be achieved by man through the exercise of his 'Knowing', and of their applications to fairly simple physical and biological problems, and to immeasurably complex problems of human social organization.

It is impossible not to admire the pertinacity and skill with which the author has carried through an immense task. Hardly any readers will be able to follow him all the way because the specialized knowledge demanded is too great, but most intelligent readers will find much to interest and stimulate them. Dr Monsarrat is probably much too uncritical of views which appear to him to fit in well with his general picture, and he is bound to slur over many of the great difficulties. He does not define his terms very clearly, and, as has been said already, the actual psychology of idea-formation is not really discussed at all. Nevertheless he has accomplished an enormous task, and the effort to relate and read some general meaning into large tracts of data which commonly are considered only in a specialist manner was well worth while.

The American Criminal. By ERNEST ALBERT HOOTON, with the collaboration of the Statistical Laboratory of the Division of Anthropology, Harvard University. Vol. I: *The Native White Criminal of Native Parentage.* Harvard University Press. 1939. Pp. 309, with an appendix of tables. $10.00.

"This", say the publishers, "is the first of three large monographs which will present fully the materials and results of a survey of race and nationality in relation to crime in the United States, carried out by the division of anthropology of Harvard

University with the cooperation of the Massachusetts State Department of Mental Diseases." The work is claimed as "the most extensive anthropological study of criminals ever made", and it certainly appears that the claim is not exaggerated. Tremendous in size as the work is—a great amount of space is occupied by the very complete presentation of statistical tables—it is throughout very clearly and cleverly written and is full of interest. There is a good historical introduction in which Lombroso receives a certain amount of well merited rehabilitation, and Goring's magnificent work is treated with discriminating sympathy. The general bulk of the discussions concern sociological and morphological characteristics, with considerable reference also to topographical, occupational and racial factors. Everything is admirably summarized at the end. The English price of the work is 42*s*. (Oxford University Press), and it seems highly desirable that a somewhat cheaper edition should be published, in which only a selection of the statistical material need be given. The full monograph contains 400 or more pages of tables.

Crime and the Man. By ERNEST ALBERT HOOTON. Harvard University Press (London: Oxford University Press). 1939. Pp. ix+403. 16*s*. net.

This is not a summary of the same author's *American Criminal*. It is an account for the general reader of the whole investigation of which that work presents only one-third. It is free from tables, but contains a lot of highly ingenious drawings. If any reader extracts more from these drawings than is meant it will be his own fault, for precisely what is meant is clearly explained in the Preface. The work deals with crime and criminals in North America. It is informed with fact and written with the most admirable good humour and incisiveness. It is rather sociological and morphological than psychological, as psychology in this field has usually been understood. But it is most highly desirable that it should receive serious study from psychologists. It does make proposals and make them incisively. And it does present an aspect of the problems involved which people who are wholly preoccupied with individual studies very often and somewhat disastrously overlook.

Personal Aggressiveness and War. By E. F. M. DURBIN and JOHN BOWLBY. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1939. Pp. viii+154. 5*s*. net.

This book contains an essay which appeared originally in 1938 as part of a symposium entitled *War and Democracy*, edited by E. F. M. Durbin and G. E. G. Catlin. The argument of the main part of the essay is that the inherent aggressiveness of the individual is thwarted and repressed when it runs contrary to the dictates of society. The pain of this repressed aggressiveness and the feelings of guilt which arise from it are relieved by displacement and projection upon some real or imagined enemy of the social group, the scape-goat; and the tendency is enhanced by the 'animistic' tendency to attribute disasters and misfortunes to the antagonism and evil machinations of certain persons—among primitive tribes of evil spirits, and in modern civilized societies of Jews, capitalists, etc. This thesis is most ably exposed, and is well supported by the evidence adduced in the appendix, which has been drawn from the psychological study of children, from psycho-therapy, from anthropological studies of primitive societies, etc. Unfortunately, though the diagnosis of the disease seems convincing, the remedies suggested—tentatively, it is admitted—are less so. When the authors pass from observation to prediction, and attempt to assess the relative values of pacifism *v*. collective security, and the affectiveness of various methods of educational treatment, they seem to lose their objectivity and psychological insight. Social psychology is not yet in a state sufficiently advanced to allow of reliable predictions as to the behaviour of the complex groups of modern civilized society.

Causes of Crime: Biological Theories in the United States, 1800–1915. By ARTHUR E. FINK. Philadelphia: University of Pennsylvania Press (London: Humphrey Milford). 1938. Pp. xi+309. 14*s*. net.

This is an interesting and well-documented study of theories about the causation of crime current in the U.S.A. throughout the period under review. It shows a general development from dogmatic and general theorizing to a more scientific and, in many ways, a more individualistic type of approach. The author shows also that towards the end of the period the individual studies are complemented by the very widespread use of statistical techniques. The general upshot of the whole survey is that the criminal is now to be regarded neither wholly as a product of environmental forces nor as a result of inborn biological factors. He is not to be characterized simply by making a list of composite traits, but must be considered, like any other person, as a 'functioning, integrated' personality. A very excellent bibliography is included.

Taste and Temperament. By JOAN EVANS. London: Jonathan Cape, Ltd. 1939. Pp. 128+48 illustrations. 10*s*. 6*d*. net.

The author of this book advances an interesting and suggestive theory, namely that artistic taste is a function of individual temperament and that each temperamental type creates and appreciates its characteristic form of visual art. But persuasively as it is put forward, the thesis seems to be entirely speculative. The particular typology selected—that of quick and slow introvert and extrovert—is not based upon any kind of enquiry or investigation into individual temperament or reaction to art forms, but upon a rather superficial study of the typologies previously suggested by psychologists and others. The analysis of the various art forms and of their manner of appeal is masterly; and the evidence as to the particular form most characteristic of the artists of a given temperamental type is fairly convincing. But it does not seem to follow that there is the same correspondence of taste and temperament in artistic appreciation as in artistic creation. Indeed, the Jungian theory might lead us to suspect the opposite. But even though the author's argument is largely speculative, it does lead to the valuable conclusion that it is useless to try to persuade people, and especially children, to appreciate forms of art to which they are temperamentally antipathetic. Unfortunately the other suggestions cannot be followed up until more is known of temperamental types and of their characteristic tastes.

The book is beautifully printed and illustrated.

Practical Child Psychotherapy. By CURT BOENHEIM. London: John Bale Medical Publications, Ltd. 1938. Pp. 177. 10*s*. 6*d*. net.

Starting from the standpoint that "the body of the child is largely the centre around which conflicts and their resulting neurotic reactions appear to be grouped", the author describes the diagnosis and treatment of the various somatic disorders in children which are mainly or partly psychological in origin. He stresses the importance of observing and where possible modifying environmental difficulties; and also of allaying the actual somatic symptoms with drugs, rest, exercise, etc. Various methods of psychological treatment are also advised and described, but rather sketchily. In fact, as its title implies, the book will probably be of more value to the medical practitioner than to the lay psychologist or psycho-therapist. But its usefulness is somewhat impaired by the stilted and involved language of the translation. A good bibliography is appended, though it is suggested that more English translations of German books might be included.

Modifications proposed for British use of the Revised Stanford-Binet Tests of Intelligence, in Measuring Intelligence, *by Lewis M. Terman and Maud A. Merrill.* By THE SCOTTISH COUNCIL FOR RESEARCH IN EDUCATION, 46, Moray Place, Edinburgh, 3. 1939..

The Scottish Council for Research in Education have prepared duplicated copies (Copyright, All Rights Reserved) of their proposed modifications in the procedure and scoring of the new Revised Stanford-Binet Tests of Intelligence (Terman and Merrill), designed to render them more suitable for British children. Copies are available to accredited testers on application to the Research Council. These modifications have the approval of Prof. Terman and Dr Merrill. They agree with some of the modifications suggested by the English Committee under Prof. Burt, but do not go as far as these. The original standardization is retained.

The Nineteen Thirty Eight Mental Measurements Year Book of the School of Education, Rutgers University. Edited by O. K. BUROS. New Brunswick, Jersey, U.S.A.: Rutgers University Press. 1938. Pp. xv+415. $3.00.

This book, which is published to some extent as an experiment, gives a very complete and comprehensive list of descriptive references to current tests and their uses. It deals also with Mental Measurement books, with research and statistical methodology books, with regional programme reports about testing technique and practice, and gives both a periodical directory and index and a publishers' directory and index. There are also full indexes of all titles dealt with and of all names of authors referred to. But the book is more than just a list. All the articles and books dealt with are briefly described, and in general the descriptions are very clear and good. Further, in the cases in which articles and books have been reviewed, a selection of the reviews is abstracted and their relevant comments indicated. Consequently it is possible to get a fairly good idea both of the matter of the publications listed and of its value as a contribution to psychological theory and practice. The amount of work involved, both on the part of the editor, and on the part of his many contributors, must have been very great indeed. It is to be hoped that the book will find a place in the libraries of all psychological departments and research centres, and that the editor will be encouraged to continue a task which, though it must be arduous, will be of undeniable service to all students who wish to engage in research in the general field covered.

Foundations of Educational Psychology. By PETER SANDIFORD. London: Longmans, Green and Co., Ltd. 1938. Pp. xv+464. 12*s*. 6*d*. net.

The foundations of educational psychology seem to be mainly biological and physiological, with a superstructure firmly built up from biometry, psychometry and Watsonian behaviourism. A rather airy top floor is constituted by the nature of intelligence and personality, but the Freudians and the Gestalt psychologists have been driven into the attics, and the child study of Buhler, Isaacs, etc., seems to have been thrown out of the window. The specialist would find himself at home in the lower part of the building, and indeed might employ himself usefully there (the study of the inheritance of mental characteristics is a valuable one). But the upper part is more suitable for the tyro. This method of construction may be instructive to the experimental psychologist; is it really helpful to the student of education?

Manhood in the Making. Edited by T. F. Coade. London: Peter Davies. 1939. Pp. xi+347. 10*s.* 6*d.* net.

There has been recently a rapid spate of books consisting of heterogeneous collections of essays or articles by various authors, with or without some attempt at editing into coherent wholes. The editor of this latest example seems to have endeavoured to select his authors in accordance with a pre-arranged scheme, so that they might demonstrate exhaustively the various aspects of the boy's development, in relation to his environment and his education. Unfortunately no connected system of ideas is ascertainable running through the various essays; and contradictions are frequent and unexplained. Moreover, although many of the essays, taken singly, are interesting, their cumulative effect is wearisome; for *ex cathedra* pronouncements are as a rule unsupported by evidence, and vague generalizations yield few concrete and specific examples. There are of course certain honourable exceptions to these strictures, of whom Prof. Burt affords the most striking. But we are forced to conclude that this type of book is of doubtful value unless there is some real collaboration, or some fundamental unity of ideas, between the different authors.

The Rediscovery of Man. By Henry C. Link. London: Macmillan and Co., Ltd. 1939. Pp. ix+257. 7*s.* 6*d.* net.

Describes how young men and women, by playing competitive games, can become good mixers, and hence Make Good.

A Study of Jealousy as differentiated from Envy. By T. M. Ankles. London: Arthur H. Stockwell, Ltd. 1939. Pp. 112. 3*s.* 6*d.* net.

This book contains the account of a piece of research work carried out for the Academic Postgraduate Diploma in Psychology. The material was obtained in the main by asking casual acquaintances to say what they 'felt' by jealousy, and encouraging them to introspect on this subject; in every case personal experiences were described, although they were not asked for directly. This material was then interpreted by the author, on a more or less Freudian basis. Both procedure and conclusions are interesting and suggestive, though possibly too superficial for so difficult a subject. Unfortunately the author's style of writing is so incoherent and lacking in lucidity that it is impossible to evaluate his work with any justice.

The Industrial Worker: A Statistical Study of Human Relations in a Group of Manual Workers. By T. N. Whitehead. London: Oxford University Press. 1938. Vol. I, pp. 265; Vol. II, 81 diagrams with statistical explanations.

In this book Prof. Whitehead has carefully analysed the voluminous data obtained from five years' observation of a special group of repetition workers. He has already given less full accounts of this experiment in shorter papers, but the present book is definitely for the expert who wants fully to understand his methods and judge the value of his results. It is impossible in a short notice to give an adequate account of such a detailed piece of work. Briefly the thesis put forward is that statistically determined social relations modify activity as measured by industrial output. He shows that, within the limits of adaptation, physical changes in environment play an insignificant part in working efficiency compared with the part played by social relationships.

The book is interesting because it breaks new ground in the methods of social psychology, bringing it definitely into line with the statistical methods adopted by experimental psychology. At the same time it advances beyond the stage of enumerating statistical relationships by relating them to the constantly changing social relations of the members of an experimental group.

VOLUME XXX OCTOBER, 1939 PART 2

THE FACTORIAL ANALYSIS OF ABILITY

I. THE PRESENT POSITION AND THE PROBLEMS CONFRONTING US[1]

BY GODFREY H. THOMSON

Moray House, University of Edinburgh

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I. WHY DO PSYCHOLOGISTS WANT FACTORS?

I PROPOSE to set out what appear to be the main principles (some of them incompatible) of the several systems of factorial analysis which co-exist at the present day. First, however, let us ask why psychologists want to have factors at all. The following reasons are given:

(*a*) If factors can be so chosen that a few of them give a good approximation to the information given by the large number of tests, there is obvious economy in their use.

(*b*) Orthogonal factors, that is, uncorrelated traits, have the advantage for scientific thought that they are independent; and they lead to simpler formulae. It should be remarked, however, that none of the human traits naturally named by naive man are uncorrelated, and that he is usually unable to realize the independence of the factors offered him by the psychologist, unable to realize for example that a man of high v (the verbal factor) is just as likely to be a man of low as of high g.

(*c*) There is a feeling that factors may be more enduring entities than the innumerable and changing tests used to find them. They come to be looked upon as the things in terms of which tests are described, although really of course it is the factors which are described in terms of tests.

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[1] A contribution to the symposium presented at the Extended General Meeting of the British Psychological Society held at Reading in April 1939.

(*d*) It is an easy transition to look upon the factors as actual and real. It is of the nature of man to deify or reify forces and powers behind phenomena, and we are all subject to this urge, which is, I think, a large part of the explanation of why factors are so acceptable to so many of us.'

II. THE PROBLEM OF FACTORIAL ANALYSIS

The scores of each test being set off along a line, these can be given direction in space at angles whose cosines are their intercorrelations. They will then occupy a space of n dimensions if there are n tests, and the population of persons will be represented by points in this space, congregated round the origin where the man who is average in every test is situated. If standardized scores are used, the population will fall off in density equally in all directions from that point, its density contours being spheres.

The problem of factorial analysis is then to choose a set of axes or factors, preferably orthogonal or nearly so, to replace the tests as definers of the space, that is of the qualities of the persons. As there are innumerable sets available, some principles must be adopted to select one as the best. 'Best' may mean here best from some psychological, or from some mathematical, point of view, or possibly these may click into agreement in some system acceptable to psychologists and mathematicians alike. Among the principles which have been adopted are the following:[1]

(i) Representing the experimental facts (within the limits of the errors present) by a smaller number of factors than tests. There is probably unanimity on this point, but not on what the smaller number of factors are to be required to do best.

(ii) Reproducing as much as possible of the *whole variance* with each successive factor (Hotelling (6)).

(iii) Reproducing the *correlations* with the minimum number of common factors (Spearman (9), Thurstone).

(iv) Insisting on a *general factor g* (Spearman, Holzinger).

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[1] I have placed after each, as a guide to the reader, the names of one or two of those most closely identified with the principle, but the classification is not of course perfect or complete. Kelley's name (7) will be missed. He is very important, but I have not felt able to identify him sufficiently with any of these principles. Stephenson's name can accompany Spearman's wherever it appears, and he is also interested in the relationship between test factors and person factors (10), though he does not make a principle of their reciprocity as Burt does.

(v) *Rotating* the factors until they become psychologically significant (Thurstone (11, 12)).

(vi) Requiring '*Simple Structure*', i.e. certain mathematical relations between factors and tests in a battery. These requirements have led to *no general factor* being found in tests reputed to be saturated with g (Thurstone (11, 12)).

(vii) Requiring *invariance of analysis* of the same test in different batteries, when used on equivalent samples of persons.

(viii) Using factors and loadings which are *reciprocal* for persons and tests (Burt (2, 3)).

III. REPRODUCING THE VARIANCE

The natural desire of the statistician (e.g. Hotelling) is to lose as little information as possible. He adopts therefore the second principle, using the principal axes of the ellipsoids which result from the density spheres when the test lines are pulled into orthogonality (6). It will of course require as many factors as there are tests, to reproduce all the information. But if, after a few factors, the greater part of the variance has been accounted for, the remaining factors are neglected. The statistician has then arrived at the best shorthand description of *this* battery. He makes no claim to have arrived at factors which have any other significance, nor that they are invariant when tests are added to or taken from the battery. His axes indeed are not factors at all in the sense intended by those who use principles (v) and (vii).

There is, however, nothing to stop a worker who proceeded on these lines from then adopting principle (v), and rotating his few main factors in their own space in the hope of making them psychologically significant; but this has not in practice been done with 'principal components' based on the whole variance.

The first few factors found by the above method will approximate both to the total variance and also to the correlations. But it is the former they do well.

IV. REPRODUCING THE CORRELATIONS

(1) *Minimal rank methods*

Better approximation to the correlations can be made by another method, which depends on the fact that a matrix of correlation coefficients between mental tests, when suitable numbers are inserted in its diagonal, is of low rank, within the limits of error. The reason for this is, in my belief, the complexity and plasticity of the mind, and the operation of the laws of chance. But whether this is the true reason or not is immaterial to our present argument, for the phenomenon is undoubtedly present and the result is that the correlations can be closely imitated by a small number of factors compared with the number of tests. This small number of factors does not however by any means give a good approximation to the whole variance but only to certain fractions called 'communalities'. There is therefore much information lost by using only these common factors. The other factors—specifics—are not needed to imitate the correlations of this particular battery. But they may be of importance in predicting success in some occupation. These minimal rank methods, therefore, which make much of the fact that they are parsimonious in common factors, attain that parsimony only by contenting themselves with imitating the correlations, and giving up any attempt to reproduce all the information contained in the tests. Their second great weakness is that since the whole number of factors exceeds the number of tests, each is subject to an indeterminacy and can be estimated with less accuracy than can one of the principal components of the former method.

(2). *The Spearman school*

Among those who use the minimal rank method there are two schools, that of Spearman (9), who discovered the fact of low reduced rank many years ago, and that of Thurstone (11). The method of the former school is to proceed by purifying a battery of intelligence tests until one general factor g is sufficient to account for the correlations. The g-saturations of the tests in this battery are then known. In more complex batteries this g is always taken out first. When a second factor is suspected, a battery is built up to measure it (by the residuals left after the removal of g), a battery which contains only these two. And so the process goes on step by step, a complex matrix of correlations being looked upon as made up by the superposition on top of the g-hierarchy, of sub-hierarchies each due to a new group factor. Invariance of analysis can be gained (principle (vii)) by retaining for any test that g-saturation which it had in the best 'g-battery' and so on with the other factors. No rotation is necessary.

Holzinger's Bifactor Method (4) is the same in principle, although it proceeds on more wholesale lines.

(3) *The Thurstone school*

In contrast with the step by step Spearman method, the Thurstone method is a simultaneous one. No attempt is made to reduce any battery by purification to rank one. Instead, a large battery is analysed simultaneously into a number of common factors—plus the specifics characteristic of both schools—by a method which involves first of all guessing the communalities and then taking at each stage the centroid of the residues. The resulting factors are devoid of psychological significance and have to be rotated in a search for positions where they will have such significance.

Here Thurstone introduces the idea of 'Simple Structure', a mathematically defined position of the axes (11). It is his belief that if in any battery 'Simple Structure' can be attained the factors will then be recognizable as psychological entities. 'Simple Structure' is marked by the absence of negative saturations and *the presence of many zero saturations*, for it is laid down that there must be at least one zero saturation in each test, and several zero saturations in each factor. This excludes a general factor, and Thurstone naturally therefore does not find one (12), even when he analyses tests said to be highly saturated with *g*. Just as naturally Holzinger and Harman (6), analysing the same data, do find a *g*, its presence in their pattern being, as they frankly say, "due to our hypothesis of its existence".

Alexander (1) has carried out an analysis using Thurstone's way of arriving at a set of minimal rank factors, but not his hypothesis of 'Simple Structure'. Instead, Alexander at this point passes over to the Spearman plan, and adopts for some of his tests analyses already known. This throws into relief the fact that the essential difference between Thurstone and Spearman is in the former's use of 'Simple Structure'. Spearman has no need, in his step by step method, to rotate his factors. But if he did use Thurstone's first analysis (as Alexander has done) he could then rotate to positions corresponding to the results of the step by step procedure. If, however, a battery of tests was put together from Spearman analyses, and offered for alternative analysis via 'Simple Structure', it would be necessary, to give a chance of agreement, to include several tests with nearly zero saturation in *g*, otherwise *either* 'Simple Structure' would not be attainable, *or* the two methods would be foreordained to disagree.

V. RECIPROCITY OF TESTS AND PERSONS

The last principle (reciprocity of persons and tests) has been put forward in its rigorous form by Burt only (2, 3), although Stephenson (10) has proposed to check analyses of tests and of persons by one another. Burt holds that those factors are the proper ones to use of which it can be said that the same results are arrived at whether we analyse tests or persons—the factors and loadings of the one analysis being the loadings and factors of the other. This end however Burt attains only by removing from the operation of the principle a factor consisting of the average performance of each person in all tests, and by working with unstandardized scores and covariances instead of correlations. Actually he removes his 'average' factor not by analysis but by selection of cases equal in average score, and it is not at all clear that he would obtain the reciprocity he desires were he to do this first for persons selected as equal and secondly for tests selected as equal.[1] On a matrix of scores which has somehow been centred both ways his reciprocity principle however does work. Such a matrix has only $n-1$ dimensions (one factor, the average, having been removed) and Burt uses the principal axes of its ellipsoids. His factors are therefore free from 'indeterminacy'. He does not however rotate them but interprets them as they are with their numerous negative loadings, a procedure which has passed muster in the analysis of temperament to which alone so far he has applied it, but would arouse opposition were it applied to the analysis of more ordinary mental test scores. His use of raw scores just as they stand is certainly wrong: but if a suitable set of units could be discovered—I have suggested one based on the sampling theory—the practice of analysing covariances instead of correlations would have much to commend it.

VI. VARIOUS OTHER MATTERS

There remain several matters which I have space only to mention. For a long time workers were content to analyse batteries of tests into factors, and did not proceed to the practical estimation of these factors in individuals. Now that this is beginning to be done, a realization is growing of how uncertain such estimates are, as a result of the mathematical weakness of postulating in minimal rank systems more factors than tests. It is also coming to be realized that to estimate factors, and from these to estimate proficiency in an occupation, is a roundabout procedure when the latter can be estimated direct.

Finally, much has been done to illustrate the influence on factors of selection among the persons, and also of training and maturing. In connexion with selection it has been made clear that factors are bound to change, emerge, and disappear with the changing sample, and therefore also with the generations of man, and Price (8) has indicated how homogamy can influence the factorial make-up of successive generations. Factors are statistical coefficients, changing with the sample and the conditions and dependent upon stated assumptions: but with defined conditions and assumptions they are most useful as descriptive terms.

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[1] This was written in November 1938 and is, as I now think (February, 1939) too sceptical on my part. I would like to withdraw lines 33 to 35 on page 290 of my book *The Factorial Analysis of Human Ability*, for I now see that since Burt chose his subsample of persons to be not only equal in average to one another, but equal to the average of all, those lines are irrelevant.

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THE FACTORIAL ANALYSIS OF ABILITY

II. DETERMINATION OF FACTORS[1]

By C. SPEARMAN

I. *Disappearance of 'G'* (p. 78).
II. *Grounds for determining factors* (pp. 78–80).
III. *Mechanism of producing zeros* (pp. 80–82).
IV. *Multitude of factors* (pp. 82–83).

I. DISAPPEARANCE OF 'G'

AFTER the admirably broad and impartially expositive paper of Prof. Thomson, apologies seem needed for now descending to a matter of comparatively narrow scope and controversial status. But to some readers an intervention on my part will not be surprising, when they learn that the point at issue is nothing less than the existence of a general factor in ability; 'G', as it has been called.

Many a time already, no doubt, this factor has had to endure rude onslaughts. But hitherto all such attacks on it appear to have eventually weakened into mere attempts to explain it more simply. Now, however, there has arisen a very different crisis; in a recent study, nothing has been found to explain; the general factor has just vanished. Moreover, the said study is no ordinary one. Alike for eminence of the author, for judiciousness of plan, and for comprehensiveness of scope, it would be hard to find any match for the very recent work on *Primary Mental Abilities* by L. L. Thurstone. With great interest, accordingly, we proceed to inquire by what method he arrived at such a result, so opposed to all those gained previously, even by himself. To some extent this question has already been touched by Prof. Thomson. We will here take the liberty of treating it a little further.

II. GROUNDS FOR DETERMINING FACTORS

Let us commence by considering the general operation of factorizing. Here, the initial data consist in the correlations between some selected variables, such as mental abilities; and the task is to infer the factors by which these correlations may be explained.

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[1] A contribution to the Symposium presented at the Extended General Meeting of the British Psychological Society held at Reading in April 1939.

Now, the great difficulty in the inference is that the data are insufficient; as is well known, one and the same set of correlations can always be factorized in an infinity of different ways. On what grounds shall the choice be made between these?

Such preference appears to be regulated by two general principles; that of statistical simplicity, and that of psychological significance. Each principle may be applied in various manners.

As an outstanding instance, let us take the determination involved in the theory of Two Factors applied to abilities whose inter-correlations present the arrangement which has been called "hierarchical". Of the infinitely diverse possible factorizations of such a set of abilities, the said theory has selected that which derives all the correlations from one single factor. Statistically, this determination is grounded on its extreme simpleness. Psychologically, it is credited with affording the sole base for such useful concepts as those of 'general ability', or 'I.Q.'

But as soon as the abilities at issue begin to deviate from the hierarchical arrangement, then the reduction to a single factor becomes impossible, and so the whole situation changes. One might think, indeed, of replacing the reduction to a single factor by a reduction to as few as possible. But if this multiple reduction be adopted throughout—that is to say, without any additional restriction—then the determinateness is really forfeited. For whereas the reduction to a single factor can only be effected in one way, a reduction to two or more can be made in an unlimited number of ways.

The lost determinateness can, however, be more or less regained by stipulating that the weights of the respective factors should be heaped up in order of calculation; that is to say, the first factor is given the largest possible weight, the second factor as much as then still remains possible, and so on. Some such determination has been employed by Hotelling, Kelley and Burt. The trouble this time is not so much statistical but psychological; the procedure generates a large and regular number of negative values which contradict all experience.

The contrary may be said of a further ground that has been employed in the quest for determinateness. It was introduced, apparently, by Alexander and is now adopted by Thurstone himself. It consists in rejecting all factorizations which, like those just considered, introduce negative weights. Psychologically, as we have just seen, this rejection is well founded. But statistically the indeterminateness is not eliminated; only decreased.

Very dissimilar, however, is another ground which has been adopted by Thurstone *additionally* to the preceding one. It is a new form of the statistical principle of simplicity. He writes: "the simplest order has been found when each row of the factorial matrix has at least one zero and when the number of zeros has been maximized."

And here at long last, not in anything we have seen hitherto, we encounter the cause of the unexpected disappearance of the general factor. Thurstone works in two stages. He starts off with his centroid procedure, so applied as to heap up the weights in order of calculation; so far the general factor is dominant, if not even exaggerated. But then he introduces his rotation of axes so as to: (*a*) eliminate all negative values, and (*b*) maximize the number of zeros. Here (*a*) is still harmless enough. But (*b*) is revolutionary; in particular, it alone is responsible for the general factor being eliminated.

III. Mechanism of producing zeros

Now, such a great and potent innovation in the method of determining factors would appear to deserve the keenest inquiry into its precise mechanism. Thurstone himself, however, tells us surprisingly few details about it. Departing from his usually irreproachable explicitness, he only remarks: "many different methods have been used in these successive rotations, and it would be impossible to describe them all."

In such default of further information, we must do what we can for ourselves. And for this purpose we will employ a few very simple examples.

Let the following figure represent a factorial matrix where the a's are the entries for four factors:

$$\begin{array}{c|cccc|} & \alpha & \beta & \gamma & \delta \\ \text{Ability I} & a_{1\alpha} & a_{1\beta} & a_{1\gamma} & a_{1\delta} \\ & \cdot & \cdot & \cdot & \cdot \end{array}$$

As our first case, we will suppose the column headed by $a_{1\alpha}$ to have the largest possible total value; $a_{1\beta}$ the next largest possible; whilst $a_{1\gamma}$ and $a_{1\delta}$ approximate to zero.

Portraying the same absolute values in a profile we get Fig. 1. Here the weights of the four entries are given respectively by their ordinates α, β, γ, and δ.

This case, obviously, belongs to the type that we have characterized as that whereby the weights are heaped up as much as possible. To this extent it agrees with the result obtained by Thurstone in his first step.

But now these weights, whilst still retaining conformity with the given correlations, admit of infinitely varying redistributions; this can be done, either by rotations of the axes, or otherwise. One such redistribution is that which may be termed a levelling-out. The ensuing profile is as in Fig. 2. And as far as the levelling is concerned, we now find an agreement with Thurstone's second step. For by that, too, the weights from first to last maintain a surprisingly uniform value.

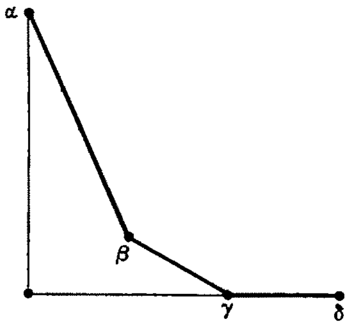

Fig. 1.

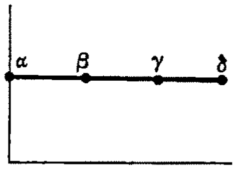

Fig. 2.

Here, however, the agreement of Fig. 2 with Thurstone's result breaks down. In the figure, there is no absence but rather an abundance of general factors. And the number of zeros, far from being maximized, diminishes from two to none.

Accordingly, to render this levelled-out profile more fully congruent with Thurstone's result, some further influence must be postulated. And this appears to be supplied in the fact that his method did not—and could not—demand the maximum number of exact zeros, but only of weights 'near' to this value. These are the weights which, since they fall below a certain minimum, are taken to be zero in the sense of 'not significant'; they are ascribed to mere errors (in large measure, those of sampling).

But all this suggests a startling consideration. What happens, we must ask, when the weights are very small throughout whereas the errors are very large? The eventual result must be that all the weights become less than their errors, so that they are all counted as 'zeros.' In general, then, we see that very numerous zero loadings may after all be merely the effect of very large experimental errors. And just this unfortunate issue would seem to have befallen Thurstone.

Accordingly, we cannot well escape from the suggestion that his otherwise eminently admirable work has what the French call "the defects of its qualities". In the first place, the extraordinary comprehensiveness of his tests left but scant time for each of them.

Hereby the correlations have necessarily been diminished. And secondly this same multiplicity of tests rendered the labour of calculating the ordinary coefficients of correlation prohibitive. He had to fall back on the tetrachoric coefficients, whereby the errors are trebled.

IV. MULTITUDE OF FACTORS

So far, out of all the treatments according to which the factor-weights could be distributed at will and so rendered determinate, we have only considered two special types; they are those of heaping-up and of levelling-out. Yet a third type also must now be brought into consideration. It may be called that of scattering. For here the weights, without altering their pattern otherwise, are scattered among more numerous factors.

If now this additional treatment be applied to our profiles, their likeness to the results of Thurstone becomes still further increased. This is true both of his initial heaping-up and of his final levelling-out. These two results of his, although so widely different otherwise, nevertheless agree in that the factors introduced are extremely numerous; no less than twelve.

Now, there are grounds for regarding this multitude with suspicion. I have myself shown elsewhere that all his results can be just as well explained with only half the number[1]. Moreover, he concedes that for five out of the twelve—although quite as large as the others—even he himself cannot suggest any psychological meaning. Yet again, we may note that when one and the same total weight is divided up into more numerous portions, these must individually become smaller, and therefore more than ever submerged in error. And so we begin to fear that the numerousness of the factors is after all only due to the arbitrary postulate of numerousness of zeros.

Anyway, the introduction of multitudinous factors certainly has an all-important consequence. However large a general factor may really be present, the share of it devolving on any single one of the divided-up factors must tend to become unnoticeable. Statistically, it will be 'insignificant'. And so at last the whole mystery of the vanishing general factor would appear to be explained. All the available evidence consistently supports the view that here this factor only ceases to be visible because of being divided up into very small pieces, which are then submerged in large errors.

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[1] "Thurstone's work re-worked." *J. Ed. Psychol.* (1939), xxx, 1–16.

By what further information such cases of its mere submersion can be still more crucially distinguished from those of its genuine absence—this is a nice problem which must be reserved for a later occasion.

Anyway, the whole course of the preceding considerations has emphasized the fact that *no* hard and fast procedure of factorizing is adequate. Since the correlations fail to determine the factors completely, some further grounds are needed for this purpose. And only in one special case, apparently, is any such ground ready to hand. It is the case of reducibility to a single factor. Here the ground is the very cogent one of parsimony. In all other cases, to find adequate grounds is a matter of no small difficulty; every case must be treated on its own merits.

THE FACTORIAL ANALYSIS OF ABILITY

III. LINES OF POSSIBLE RECONCILEMENT[1]

By CYRIL BURT

I. *The meaning of factors* (pp. 84–85).
II. *The sampling theory and the two-factor theory* (pp. 85–86).
III. *Interpretations of G* (pp. 86–89).
  (a) *Sampling the neural bonds* (pp. 87–88).
  (b) *Sampling the tests* (pp. 88–89).
IV. *The reciprocity principle* (pp. 90–92).
  (a) *Variations in variance* (p. 90).
  (b) *Negative saturations* (pp. 90–92).
V. *Special issues* (pp. 92–93).
References (p. 93).

PROF. THOMSON, in a paper as concise as it is impartial, has covered almost the whole field of factor analysis. Prof. Spearman in his defence of the general factor and Dr Stephenson in his paper on Q-technique have raised problems more specialized and distinct. It has been suggested that I should deal mainly with two fundamental issues: (i) the sampling theory as opposed to the two-factor theory, and (ii) the reciprocity of tests and persons. Even so, in the space allowed, I can touch only upon one or two outstanding questions, with a curtness they scarcely deserve.

I. THE MEANING OF FACTORS

Much apparent disagreement would vanish if we all kept to the primary meaning of the term factor. The matrices that define factors in psychology have the essential properties of 'selective operators'. Hence a factor is primarily a principle of classification and nothing more: it is expressed in quantitative form simply because the items, whose characteristic pattern constitutes the distinguishing mark of the class described by it, vary continuously and in degree rather than discontinuously and in kind. Now, just as the traditional logic of classes has recognized four kinds of possible predicable—genus, species or differentiae, individual propria, and accidents—so factor analysis is led to envisage four kinds of possible factors—general, group or difference, specific, and accidental ((6), p. 259). The two-factor, three-factor, and multiple-factor theories are all special derivatives of this four-factor theory. And most of the difficulties and controversies that trouble the factorist in psychology have already been debated at great length by different schools of logic. The temptation to identify an abstract and hypothetical factor, reached by mere mathematical analysis, with a real and concrete 'factor in the mind' (e.g. with an 'ability') is simply a new instance of the old problem that has divided nominalists, realists, and conceptualists for centuries. So long as we regard the factors reached by Spearman and Thomson as descriptive components only, and not as causal entities, there is (as I hope to show) no serious conflict between their theories.

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[1] A contribution to the symposium presented at the Extended General Meeting of the British Psychological Society held at Reading in April 1939.

II. THE SAMPLING THEORY AND THE TWO-FACTOR THEORY

Thomson's sampling theory, like Spearman's two-factor theory, was originally put forward to explain the 'hierarchical order', which Spearman was the first to notice, among the coefficients obtained by correlating cognitive tests. The sampling theory maintains that "the hierarchical order is the natural relationship among these coefficients". I agree; but on more general grounds. By a hierarchy I assume we mean a matrix of rank one.[1] Now, as I have elsewhere shown ((7), p. 187), the process of self-multiplication, if sufficiently repeated, will ultimately reduce *any* matrix to a matrix of rank one. But the process of correlating is essentially a process of self-multiplication. Hence to correlate is to take the first term in a converging sequence of matrix products which must eventually end in a hierarchy as perfect as can be desired.

To my mind, therefore, the question is not: Why are our correlation tables hierarchical, i.e. why have they only one latent root, so that the reduction seems virtually completed in a single step by the sheer process of correlating? Such cases were nearly always the illusory effects of high probable errors. The question should rather be put: Why is the first latent root so much larger than the second, and *a fortiori* larger than the rest? My answer is: Because of the way in which the psychologist has selected his tests. Examine the correlation tables cited as evidence of a marked hierarchical tendency: it will usually be found (i) that the tests were all chosen from one generic type, in most cases cognitive; (ii) that only positive examples of the type were taken, i.e. the tests always measured ability, never disability; further, that the tests were picked to represent, as far as possible, (iii) widely different subtypes, and (iv) widely different levels of complexity; but that, at the same time, (v) each subtype, and usually each level, was represented in most researches by one test only. These conditions of themselves are sufficient to ensure several striking features in the resulting correlation tables: we may safely infer that the correlations will be almost always positive and never (except for the accidents of sampling) negative; will range from small to high values; tend to obey the proportionality criterion, but also tend to exhibit a slight, diminishing and (as a rule) cyclic overlap. Now the conditions specified cover the essential requirements of 'random' selection. Thus the hierarchical tendency arises, not so much because the tests have blindly sampled the bonds at random, but *because the psychologist has systematically sampled the tests at random.*[1]

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[1] Not all the hypothetical tables given by Spearman and his pupils to illustrate a perfect hierarchy are matrices of rank one: cf. (10), vol. II, p. 274, and (3), p. 175 (where "it is assumed that in a hierarchy the coefficients decrease *arithmetically*", and *not* proportionately: e.g. in the table given as an example of a perfect hierarchy "the correlations in both rows and columns decrease throughout by 0·05").

III. INTERPRETATIONS OF G

There is no mathematical incompatibility between the two-factor theory and the sampling theory, as their respective advocates at one time maintained. Applied to the facts that both are concerned to interpret—the simple hierarchical tendency—the two procedures give virtually the same result. As I have tried to demonstrate by algebraic proof, if we estimate for every person the total of the test measurements to be expected on Thomson's sampling theory, those totals will possess all the properties of a general factor. Conversely, as Thomson has shown, by an ingenious transformation matrix[2] a two-factor matrix can always be rotated to give a sampling theory matrix.

The real question, therefore, is—which theory best fits what we know of the working of the mind or central nervous system? And this (in spite of Stephenson's protest against "irrelevant attempts to bring neural interpretations into the picture") is an issue that can be answered only by non-mathematical evidence. As Spearman himself has observed: "the burden of further proof is in large measure transferred to physiology".

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[1] Lack of space compels me to be content with unqualified and undemonstrated statements. But the interpretations put forward in §§ I and II will be at once clearer and more plausible if the reader thinks of what would occur with correlations, not between tests or traits, but between persons.

[2] The transformation matrix as it stands is non-singular. Consequently Thomson may not accept the previous statement.

(a) *Sampling the neural bonds*

Stephenson has maintained that "there are in the human being... only a few unitary traits, born out of system (1) analysis", i.e. "out of the analysis of primary abilities by *r*-technique with two-factor methods". Thomson, on the other hand, maintains that "far from being divided up into a few 'unitary factors', the mind is a rich, comparatively undifferentiated complex of innumerable influences—on the physiological side an intricate network of possibilities of intercommunication". The latter description is evidently in much closer accord with the neurologist's conception.

Spearman would identify a man's g with his mental 'energy' (should it not be 'power'?). Thomson favours the view that "the general energy, if there be such a thing, is...of an instinctive and emotional nature rather than intellective". With this, of course, I agree, since I identify 'mental energy' with 'general emotionality', not with general intelligence; the latter (as McDougall and Maxwell Garnett have argued) *directs* the general energy. Thomson's own theory, however, seems to exclude all possibility of direction.

He begins by arguing that the correlations between cognitive tests can best be explained, not by attributing them to the amount of mental energy each man has, but rather to the number of neural connexions each man can organize. This I readily accept, so far as it goes. Moreover, his further postulate of 'pools' and 'sub-pools' seems to me to be quite in keeping with that combination of general homogeneity of cellular structure with minor differentiations in different parts (i.e. in different layers and areas of the cortex), and of mass action with partial localization, that the structure and functions of the brain unquestionably exhibit. But when we ask how the different tests succeed in setting this 'structural network' into operation, Thomson's account appears somewhat inadequate. I willingly admit that a blind, trial-and-error process is set up by many of our tests (especially by tests blindly and therefore often inappropriately selected by the tester—e.g. some of the tests that have been applied to duller creatures, whether animals or human beings); but I should maintain that the higher tests of intelligence as applied to higher minds operate specifically by *selecting* the 'bonds' rather than by *sampling* them. Here, therefore, I agree with Spearman that a 'good test of g' will not stimulate neural connexions at random, but is designed to educe the particular connexions that are relevant to the test problem.

I should even go further: the eliciting of blind associations and the eduction of relevant relations are to my mind only partial aspects of the essential function of the mind or nervous system, namely, its *integrative action*. From this standpoint, as it seems to me, both theories can be reconciled.

These two theories, however, envisage only correlations between tests, not correlations between persons, and only tests of cognitive abilities, not tests or assessments for temperamental traits. But, as recent work has amply shown (e.g. (11)), when we extend the scope of our experiments to include these newer problems and methods, the hierarchical tendency still predominates in the resulting tables: or, to speak more accurately, the latent roots of the correlation matrices continue to show much the same rapidly diminishing proportions in both cases (approximately 50, 16 and 5% of the total variance, when no special selection operates). This again suggests that the hierarchical tendency (or what is loosely described as such) may be the direct effect of the process of correlating; but it does not prevent us from postulating some feature in the mind which allows this effect to manifest itself undistorted. Would Thomson accept the energy hypothesis here, to explain the positive and proportionate correlations between emotional traits? Or would he account for the so-called 'energy factor', and the other general factors that are sometimes found, by the partial overlapping of numerous elementary tendencies? When we consider the apparent nature of the factors so far revealed, it would seem more plausible to attribute the secondary factors at any rate, not to the random sampling of the elementary tendencies, but rather to the way those tendencies are organized, i.e. to certain *general attitudes or dispositions*, *characteristic of the whole personality*. If so, the integrative hypothesis will cover both fields.

(*b*) *Sampling the tests*

Elsewhere (8) I have pointed out that, just as in correlating tests we have to consider how we have selected or sampled our persons, so in correlating persons we are compelled to consider the fact that we are now either selecting or sampling the tests or traits; and much the same principle applies in the second case as in the first: i.e. the correlations between persons can be most readily interpreted if the "tests form a random sample of the total population of tests" ((8), p. 68). If we deduce the natural consequences of this condition, we are led to theorems which closely resemble Thomson's sampling theorems and his related theorems on selection, except that our propositions now refer, not to the fact that our tests are sampling or selecting the mind's neural bonds, but to the fact that *the psychologist is sampling or selecting the tests or traits.* For example, just as the factor measurements for the persons obtained by correlating *tests* measure the complexity (or 'richness', as Thomson calls it) of those persons' minds, so we might say that the factor measurements for the tests obtained by correlating *persons* measure the complexity or 'richness' of the tests. Now, Thomson has pointed out that, when correlating tests, the richness of the tests is measured by their saturation coefficients. But, if I am right in the general equivalences that I have endeavoured to establish between the two methods of approach, then, by the so-called 'reciprocity principle', saturation coefficients for the tests, obtained by correlating persons, are virtually identical with the factor measurements for the tests, obtained by correlating tests. This seems to bring my own speculations closely into line with Thomson's views.

No doubt, these deductions from the wider theory of sampling will not throw much light on the analysis of the mind; and it is rather with this latter problem that Thomson, like most psychologists, is more immediately concerned. But they certainly throw light on the important practical problems that arise in actual examining and testing (e.g. when we consider that any particular examination paper can include only a limited sample of the entire 'population' of questions that could be asked about a given subject).

On one minor point I differ both from Thomson and from Spearman. From Thomson's sampling theory it follows that, on a *single* application, a test of very slight complexity or 'richness' would give low intercorrelations and low self-correlations (reliability coefficients), but that, if repeated and corrected for unreliability, it would give as high a correlation with other tests (and therefore with the general factor) as the most complex. Spearman has similarly maintained that, after due correction, there exists "a correspondence between what may provisionally be called General Discrimination and General Intelligence which works out with great approximation to one or absoluteness" ((1), p. 284). Now in my own early experiments I endeavoured to include (if I may borrow Spearman's terminology) tests of "all forms of sensory discrimination" and also of "the more complicated intellectual activities". I found that simple sensory and simple motor tests often gave high reliabilities, and yet, even when corrected, furnished comparatively small correlations with cognitive tests of a more complex type. This seems to me to fit much more closely with a 'complexity' (or integrative) theory of intelligence than with either a sampling theory or an energy theory.

IV. THE RECIPROCITY PRINCIPLE

May I now turn to the criticisms advanced by Thomson and Stephenson against my own conclusion, namely, that the non-general factors obtained by correlating persons are the same (except for minor differences of weighting) as those obtained by correlating tests?

(a) *Variations in variance*

Stephenson's chief objection to my reciprocity principle, as he terms it, is that it implies objective differences in variance. Variance, he insists, "depends solely on the whim of the psychologist"; hence, the first step in any form of factor analysis should be to convert the raw measurements into standardized scores, and treat the variances as equal. Now anyone who has investigated individual variation for different capacities and attainments (e.g. to compare it with the variation between different ages or different sexes) quickly learns that it is by no means dependent solely upon the psychologist's whim. As I have elsewhere pointed out, there is a vast accumulation of evidence indicating that *variance depends largely, if not mainly, on the complexity of the process measured.* Indeed, the method of factor analysis that I have found most successful entails the identification of the estimated self-correlations, not with unity (as Hotelling has assumed) nor with the highest correlations for each test (as Thurstone proposes), but with a proportionate estimate of this differing variance. The implication which Stephenson criticizes has therefore always formed in my view a merit of the reciprocity principle, not a defect.

(b) *Negative saturations*

But the strongest criticisms have been directed against my acceptance of 'negative loadings just as they are'. As Spearman puts it, the 'negative values' for the saturation coefficients 'contradict experience': and with this both Thomson and Stephenson, as well as Thurstone and Alexander, would seem to agree. Factors, it is assumed, are 'abilities in the mind'; and to talk of an 'ability' as something which *hinders* a person from performing a test successfully seems a contradiction in terms. My defence is twofold: the assumption tacitly attributes to the 'factors' a meaning which is at once (i) too intellectualistic and (ii) too concrete.

(i) Performance in any test is a result of conative energies as well as of cognitive capacities. A child who devotes most of his time and interest to one subject must give less time and interest to others. Conversely, lack of ability in one subject may, by compensation, lead to added effort in another. Moreover, as experiments on transfer amply show, the acquisition of one kind of ability often interferes with the acquisition of a second. And generally, we may ask, when reciprocal inhibition and occlusion are such well established features of neural activity, as mental conflict is of higher mental processes, why should factor analysis, for purely *a priori* reasons, refuse all recognition to interference-factors?

(ii) But the negative saturations may be defended on a more fundamental ground. Strictly regarded, test measurements do not describe minds; they describe relations between minds and their environments; and the factors that result denote, as we have seen, abstract principles for classifying such relations. Now classification may proceed by logical dichotomy just as well as by positive and co-ordinate classes. Bipolar factors classify by antitheses—subjects, for example, into verbal subjects and non-verbal, non-verbal subjects into arithmetical subjects and non-arithmetical, and so on: group factors classify by co-ordinate classes—verbal, arithmetical, manual, and the like. Or, if we prefer to apply the classification to persons, the same factors will lead us to classify pupils in the same alternative ways. It is the negative saturations that arise in this second application—i.e. the negative saturations deduced by correlating persons—that appear to cause greatest difficulty to my critics: for example, in experiments on imagery, if a person is a good visualizer, the saturation coefficient for my first bipolar factor gives him a positive mark; if he is bad, a negative. Yet surely this is nothing but the common device of translating absolute measures into deviations about an average. If abilities can never be expressed by negative figures, how does Stephenson defend the negative deviations that inevitably arise when he converts his test measurements into standard measure?

The foregoing criticisms turn mainly on different views as to method. How do they affect the results? When I first proposed using the device of correlating persons for researches in which we were jointly interested, Stephenson was not alone in criticizing the assumptions which the method implied. But he went further. He put forward, among other fruitful suggestions, an alternative two-factor technique, which, he considered, would escape many of the objections urged against my 'multifactor technique'—a 'Q-technique' to replace the so-called 'P-technique'. With this modified method, he now believes, the factors that result from correlating persons, so far from being merely 'the obverse' of those that result from correlating traits (as he at first was inclined to agree), prove to be 'poles apart'. Working side by side in the same laboratory we have since been able to apply our respective methods to much the same problems and often to the same material. The reader can, therefore, examine for himself the differences in our published results. There will, I think, be little doubt about the conclusion. In every field—scholastic preferences, aesthetic appreciation, temperamental types—the factors described by Stephenson are virtually[1] the same as those obtained by the earlier and alternative techniques.

V. SPECIAL ISSUES

In reply to my criticisms of his first paper (on "*Q*-technique") Stephenson has further developed his views in a second paper (on "Abilities defined as Non-Fractional Factors"), and suggests that the distinctions between 'mathematical' and 'psychological' factors and between concrete 'abilities' ('non-fractional' factors) and abstract 'components' ('fractional' factors) will go far to meet my points. To discuss the way he redefines these distinctions and classifies cognitive processes in his table would require more than my allotted space. I will merely say, first, that I am delighted to find him now explicitly accepting the 'theorem of four factors'—i.e. four *kinds* of factors (general, common or group, specific, and error or chance factors);[2] and, secondly, that I readily accept his algebraic demonstration, since it would appear to be virtually the same as my own ((9), p. 167). On the other hand, I doubt some of the deductions that he draws, and am surprised by certain omissions from the arrangement of levels and subtypes in his scheme of test construction.[1] But it is plain that we are now in much closer agreement than appeared at the outset of our discussion.

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[1] The only large difference between our figures appears in the field which happened to form the subject of Stephenson's first experiment on *Correlating Persons instead of Tests*, namely, 'artistic taste' ((5), p. 21). Here, as one of my critics points out, Stephenson's highest correlation was only 0·66, whereas in a previous account I had given an average correlation of 'nearly 0·90' ((4), p. 280). But my figure was obtained with pictures deliberately chosen so as to differ as widely as possible in artistic merit, whereas Stephenson's was obtained with very homogeneous material—namely, simple colours: indeed, his findings should be compared, not with my discussion of the general factor of artistic taste (as shown in the experiments with pictures), but rather with my description of *types* of artistic appreciation (as shown by my own experiments with simple colours (4), pp. 280 *et seq.*). I may, however, add that in correlating persons (as was pointed out in my early work with Binet tests) the coefficients obtained are apt to look surprisingly high; and in Stephenson's more recent work the figures are much higher and approach far more closely to my own (cf. the summary in (11), p. 413).

[2] In the revised version of this passage the 'error factors' are apparently included in the 'specific', and not regarded as a separate kind of factor. It should be added that both Stephenson's discussions and my own have here been greatly abridged owing to the exigencies of space. But we have drawn up jointly a more detailed statement on the points over which we agree and disagree; and this will shortly appear in *Psychometrika*.

With Prof. Spearman's criticisms of Thurstone, I am largely in agreement, and can give independent confirmation of his chief conclusion. As soon as Thurstone's tables were available, Mr H. J. Eysenck subjected them to a factor analysis by the 'group-factor method' used in my earlier work (12). He finds (i) practically the same values for the general factor as Prof. Spearman, and (ii) practically the same non-general factors as Thurstone. Thus, once again it seems possible to reconcile views that at first appeared entirely opposed.

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[1] My own 'scheme of test construction' includes, on the cognitive side, a motor group as well as sensory groups, and associative groups (memory, habit formation) as well as relational: cf. for example, the scheme set out in (2), pp. 98–9, and later papers. But, as the earlier paper shows, I should willingly agree with Stephenson that the tests selected for factor analysis should not be (as he puts it) "any old things we care to throw together". Such a principle (or lack of principle) certainly suggests that the investigators were merely taking random samples of all possible tests; and the features of their correlation tables might therefore be plausibly regarded as the effects of such sampling. To some extent no doubt that is true. In practice, however, sampling that is not consciously controlled is not so truly random as might be supposed.

THE FACTORIAL ANALYSIS OF ABILITY

IV. ABILITIES DEFINED AS NON-FRACTIONAL FACTORS[1]

By W. STEPHENSON

I. *Introduction* (pp. 94–95).
II. *The relations between test construction and factor theorems* (pp. 95–99).
    (1) *Spearman factors* (pp. 95–96).
    (2) *The universal factor*, G_u (p. 96).
    (3) *The sense factors* (pp. 97–98).
    (4) *The material factors* (pp. 98).
    (5) *Relation and form factors* (pp. 98–99).
III. *Factor theorems* (pp. 99–100).
IV. *Abilities* (p. 100).
V. *Factors* (pp. 101–103).
    (1) *Non-fractional factors* (p. 101).
    (2) *Relation between nf and f factors* (pp. 102–103).
VI. *Conclusion* (p. 103).
References (pp. 103–104).

I. Introduction

The factorist, as psychologist, should set out to put questions to human nature. Apparently we have all very different questions to ask. Thomson asks questions about a vast nebulous sea, a protean mass, of innumerable brain elements; Burt tries to bring integration into this picture; whereas Spearman and I want to know whether memory is the same functionally as eduction, or whether abstraction is anyway different from generalization. Even as I say this it seems obvious who are asking psychological questions and who are not.

Again, except in Spearman work, I know of no factor systems in which some reference is made to the characteristics of the tests we use. Yet, so it seems to me, unless factor theorems refer in some way to the apparent characteristics of the variables we use, factorists cannot hope to speak the same language. As for the current factor methods, all the beauty and accuracy of Hotelling's procedures cannot hide their incompetency

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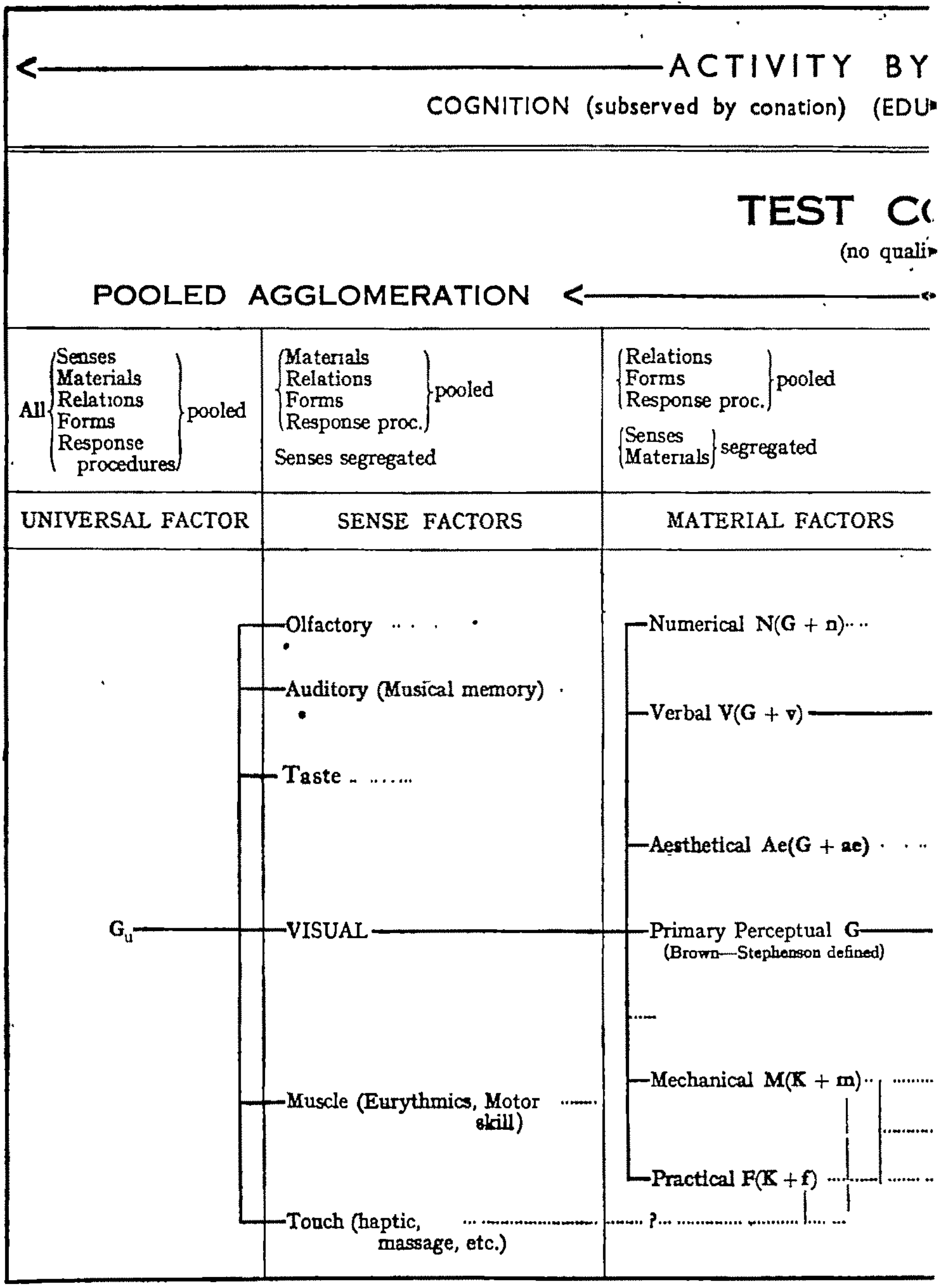
[1] A contribution to the Symposium presented at the Extended General Meeting of the British Psychological Society held at Reading in April 1939.

Table I. *Showing conne*



← ACTIVITY BY

COGNITION (subserved by conation) (EDU

TEST C

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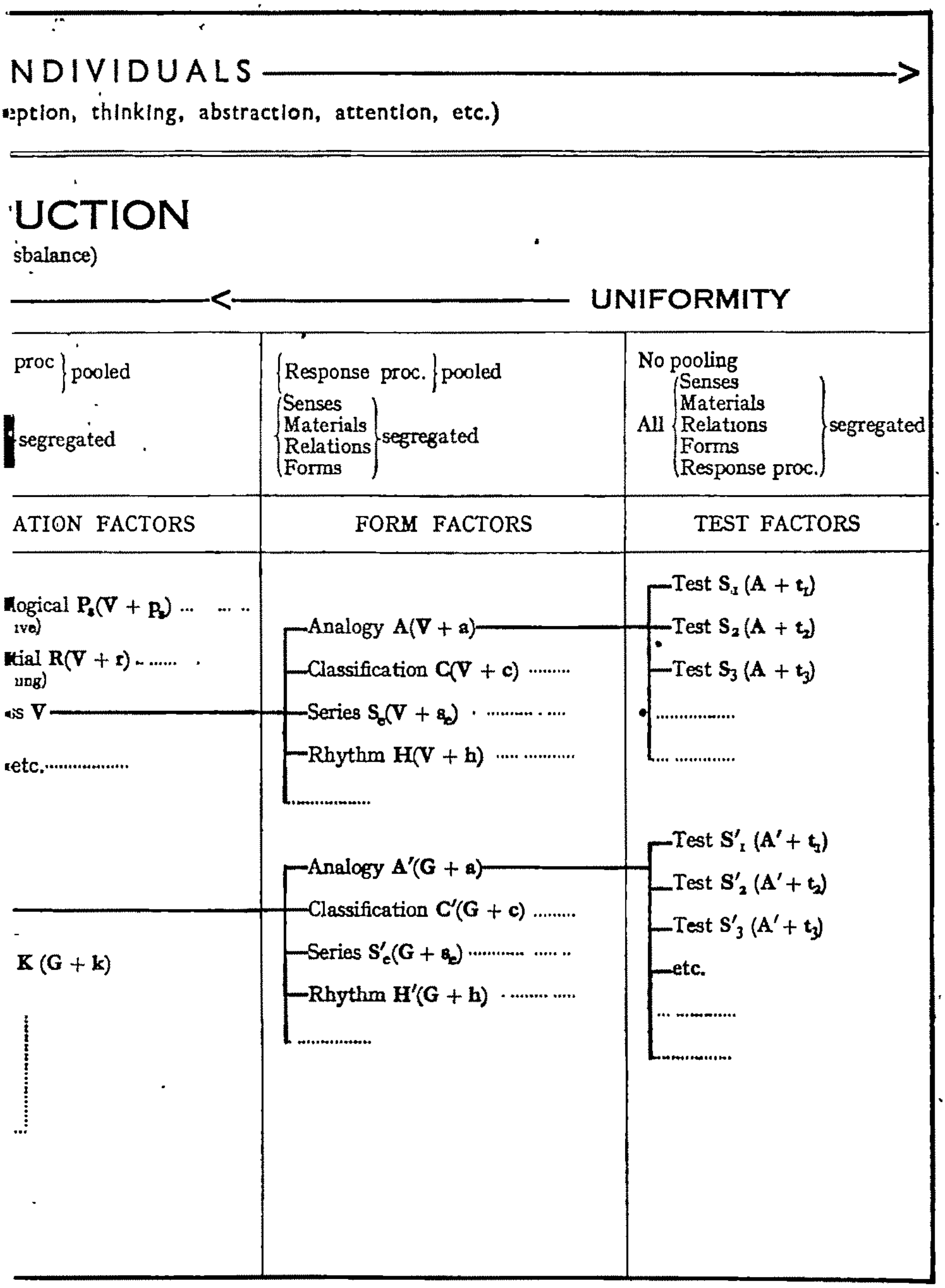
POOLED AGGLOMERATION ←

All Senses, Materials, Relations, Forms, Response procedures pooled	Materials, Relations, Forms, Response proc. pooled Senses segregated	Relations, Forms, Response proc. pooled Senses, Materials segregated
UNIVERSAL FACTOR	SENSE FACTORS	MATERIAL FACTORS
G_u	Olfactory Auditory (Musical memory) Taste VISUAL Muscle (Eurythmics, Motor skill) Touch (haptic, massage, etc.)	Numerical N(G + n) Verbal V(G + v) Aesthetical Ae(G + ae) Primary Perceptual G (Brown—Stephenson defined) Mechanical M(K + m) Practical F(K + f) ?

(*a*) Factors in capital letters are non-fractional. Factors in small letters are fractional.

(*b*) The more uniform a test is, the greater the complexity of the *nf* factor representing it.

(*c*) Spearman's universal *g* factor has so far been defined in terms of an *nf* factor for primarily perceptual tests.



(*d*) Abilities (represented by *nf* factors) can be defined at any stage of test agglomeration.

(*e*) 'Capacities', 'powers', etc., should be independent critically of test construction, i.e. of material, relation, form, or test factors.

to deal with psychological problems. Thurstone's factors, likewise, are in no way defined in terms of psychological needs. Prof. Burt's reciprocity procedure, a most notable advance in technique, is insecurely founded, for as yet he has put forward no generally acceptable system of units, which is surely of first importance in any science.

I find very little help from these various methods for the concrete psychological problems that we seek to solve, and that, in the last resort, can only be solved by systematic factor studies. Our concern, in the major part of factor work, is with the activity of individuals, the tests we construct, and factor theorems. The way in which these impinge upon one another is worth illustrating, I think, in terms of Spearman factor studies.

II. THE RELATIONS BETWEEN TEST CONSTRUCTION AND FACTOR THEOREMS

(1) *Spearman factors*

I propose to consider what are the connexions between test construction and factor theorems, and how both relate to the activities that the tests set in motion in the minds and bodies of the persons we test; I do this by referring to actual work done on, or implicit in, Spearman's Theory of Two Factors.

Table I shows a broad landscape of the components in question. Only a few of the possibilities are given in the table, which reminds us that cognitive activity is under consideration. From work already done we exclude memory, speed preference, and sense acuities from the activity in question. This leaves perception, thinking, generalization, abstraction, conceptualization and the like, and eductive processes in general. Spearman's theory applies to *any* tests whatsoever for such activities, provided the tests are sufficiently dissimilar. Any such tests define Spearman's universal g-factor (and not, as Thomson supposes, merely the tests used by Brown and myself (1)). Spearman's s factors are at the outset merely that part of the variance not accounted for by g factor. But these s factors can be subdivided and made cogent, and an essential objective in Spearman work is that of determining just what subdivisions are permissible in fact and pertinent in psychology. Table I offers some information about such subdivisions.

Instead of regarding tests as any odd things we care to throw together, Table I seeks to bring a sense of orderliness into their construction. Tests are constructed as agglomerates or pools of test-units, and the pooling takes place systematically in terms of test-units of different senses, modes of material, relations, forms, and response procedures. In Spearman work we distinguish between psychological factors such as the universal g, group factors, and specificity factors, and mathematical factors (general, common, and specific). But Table I goes further than this and tries to give some idea of the nature of certain psychological factors—instead of group factors[1] it shows *material* and *relation* factors, and instead of specificity (or minor group) factors it shows *form* and *test* factors.

(2) *The universal factor*, G_u

Systematic psychological work consists of constructing tests which endeavour as far as possible to deal with each mode of activity separately—that is, test systems can be produced in which abstraction is held critical, and these compared with others in which generalization is held critical. The activities are dealt with as variables to be controlled. But to begin with we could consider test-units for all forms of activity referred to at the head of the table. In this sense our concern will be with 'general' intelligence.

In the first column we contemplate tests for complete agglomeration—they are pools of all possible varieties of test-units, for all possible cognitive activities, senses, materials, relations, forms and response procedures. Such tests would presumably measure 'general' ability—*by which I would mean an ability in no critical way depending upon senses, materials, forms, relations, etc.* The agglomeration is no mere pool of *everything* pertaining to cognitive activity: it is devised so as to ensure no critical dependency upon the idiosyncracies of the senses, modes, forms, etc.

The variance for such tests would be divisible into

$$G_u^2 + s^2 = 1.$$

The s factor would be error, *and there could be no tangible sub-division of this into parts, except errors*, for we could not identify such parts with anything meaningful in psychology.[2] G_u is far the most comprehensive mixture of test-units that is conceivable. So far no such tests have been constructed, and no such factor isolated.

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[1] These have always been regarded as not only those which extend into several variables but those which, in addition, are of major psychological importance. It is easy to produce hundreds of *common* factors, but very difficult to arrive at even a handful of *group* factors.

[2] But when we subdivide the *activities* into different modes, these s factors can take on meaning with reference to such activities.

(3) *The sense factors*

The first subdivision would ordinarily be into the *sense* modalities involved. These would supply tests with test-units all of one *sense*, but with all possible activities, materials, relations, forms and response procedures pooled. These tests must not be confused with any for measuring sense acuities. On the contrary they exclude acuities. The tests contemplated are broader than any extant to-day, not less. Thus we might construct a hodge-podge test in which the fundaments all involve olfaction—our tests would be bottles of smells, etc., for different response procedures (selecting one bottle, perhaps, from a set of four), for different test *forms* (classification, series, etc.), for different psychological relations (especially likeness, psychological, and spatial).

Suppose a number of such agglomerate tests are made. The comprehensive pooling ensures as far as possible (theoretically) that materials, forms, relations and response procedures are not critical, and the following Spearman factors become possible:

nf factor*	Divisible into	*f* factors*
(*a*) Olfactory *O*	,,	$g^2+o^2+s_o^2=1$
(*b*) Auditory *A*	,,	$g^2+a^2+s_a^2=1$
(*c*) Visual *I*	,,	$g^2+i^2+s_i^2=1$
(*d*) Muscle *U*	,,	$g^2+u^2+s_u^2=1$
(*e*) Touch *W*	,,	$g^2+w^2+s_w^2=1$

* For explanation, see later.

Because of the gross pooling, the *s* factors could not be further subdivided meaningfully. The group factors *o*, *a*, *i*, *u*, and *w* would be particular to the respective senses. I do not say that all of these will exist (for no work has yet been done with such factors)—but any that did, could be identified in some way with sense modalities.

How would the *g* component be defined? From the internal evidence of the tests for sense segregations we might find it convenient or permissible to define it as a factor for one of the senses. For several reasons the visual sense suggests itself. Tests for visual modality would exclude symbolization and reading, in which memory, auditory and motor processes mediate. The tests would be composed of primary perceptual material—shapes, lines, colours, volumes, surface structures, etc. They would in fact be the same as those defining the material *G* factor (column three of Table I). The variance for the visual sense nf factors would then be

$$G^2+s_1{}^2=1;$$

and all the other sense-factors, o, a, u and w, etc., would be relative to this G.

(4) *The material factors*

Spearman work hitherto has centred largely on material factors. The major abilities discussed in popular psychology also come in this category. Thorndike's social, concrete, and abstract intelligences are concepts that merely embrace the materials each is concerned with—so are mechanical, artistic, practical, and scholastic abilities. All are inextricably bound up with particular kinds of material or psychological 'objects'. Table I lists factors N, V, Ae, G, M and F.

G has been identified with Spearman's universal g factor of the theory. It is the simplest thing to accept, and is in keeping with sound psychological notions. The other material factors are all divisible into G plus a group fractional factor. The factor G now plays two parts—it can be tested separately with visual perceptual tests, and also enters into all other tests. Such are the 'unitary' factors sought for by factorists. Consider, for example, V ability. Any verbal test, however narrowly segregated for form, relations, etc., will be divisible into G and v. But for sane definition of such a v component it would be wise to recognize pooling again: the tests of V ability would be agglomerates of all possible verbal relations, forms and response procedures. Can anyone doubt their efficacy when so defined? Their variance would be

$$G^2+v_1{}^2+s^2=1.$$

Again, because of the pooling, the factor s could not be meaningfully divided into smaller components. The factor v_1 would tend not to be the same as that issuing from my earlier studies on v factor (6). It would be a more comprehensive fractional component.

(5) *Relation and form factors*

So we could continue, with increasing separation of the material into that of particular relations, forms, and response procedures. I am perhaps the only factorist who has tried to attend to relation factors, through the work of Wedek (4), Murdoch (3), Koussy (2), and others. Tests particular to the critical use of one relation (objective, likeness, spatial, relations *of* evidence, and *by* evidence (reasoning)), now permit of a finer subdivision. Tests of reasoning, for example, are divisible into

$$V^2+r^2+s^2=1,$$

where

$$V^2=G^2+v_t^2,$$

and where r is peculiar in some way to reasoning. Whether the s factor is regarded as error, or given a meaningful division, will depend upon whether tests are pooled or uniform with respect to form, etc.

Factors such as *ps*, *r*, *k* do not necessarily apply to the relations as such. They may be incidental to them. Thus, Wedek could as easily identify *ps* with temperament; Murdoch's *r* might be connected with the need to "keep a large number of facts in the mind's eye verbally"—many reasoning problems are very easy when we draw or note down the possibilities and do not need to carry the details 'in our head'. Similarly, *k* factor is particular to the 'mental manipulation' of shapes—and not to space-relations as such. Such results are at the mercy of nature—we set out to find one thing, but discover another.

Still further subdivision is possible if test forms are segregated. Pooled analogy tests of psychological ability might be divisible into

$$G^2+v_t^2+ps^2+a^2+s^2=1,$$

where a now refers to the analogy test form in some way. Finally, for a particular form of analogy test, with a particular response procedure, the *nf* factor could be sensibly and meaningfully divided into

$$G^2+v_t^2+ps^2+a^2+t^2+e^2=1,$$

where t refers to response procedure.

III. FACTOR THEOREMS

To arrive at meaningful factors something of the kind I have described above seems to me to be essential. There would, along such lines, be no disorderly scramble for an 'explanation' of observed fractional factors; the 'explanations' would suggest themselves because of the very nature of the test constructions, although as always, alternative 'explanations' for any factor must be borne in mind. The segregations in Table I are initially purely of classificatory significance: it is the purpose of factor theorems to tell us whether these classifications also have functional, factor, reality. To this end I find that the Spearman *theorems* of general, common, and specific (including error) factors are sufficient for all practical purposes. These theorems are implicit in the fractional components considered above. In terms of such theorems the classifications in Table I have been shown to have a certain functional reality. This applies particularly to the *material, form* and *response procedure* factors. There is no clear evidence as yet that *relation* factors exist as such; and very little work has been done so far with *sense* factors.

IV. ABILITIES

Several details require mention, in order to deal reasonably with conditions such as Table I contemplates.

To begin with, everyone will agree, I hope, that factor work of the kind under discussion is concerned with *abilities.* Yet Thomson seems nowhere to define what is meant by an ability, even in his recent *Factorial Analysis of Human Ability* (7). Thurstone, in his *Primary Mental Abilities* (8) asks, on p. 1, what is an ability? But he supplies no answer. Instead he takes it for granted that we all know already. Earlier, in his *The Vectors of Mind* (9), he gave an explicit verbal definition—"an ability is a trait which is defined by what an individual can do". This, he added (p. 48), "implies that there are as many abilities as there are enumerable things that individuals can do. Each ability is, therefore, objectively defined in terms of a specified task." So defined, abilities are altogether too raw. They could include activity like sneezing, or feeling and orexis generally. What is required is the explicit formulation of abilities in terms of factor theorems, test construction and psychological hypotheses. Far from being the raw material for treatment by factor methods, for the discovery of primary abilities, unitary factors and the like, *it is my view that abilities themselves have to be defined in terms of the products of prior factor work.*

The nearest to a formal definition that meets my purpose comes from Alexander (10). He found it necessary to talk about 'functional abilities', which were "the resultants of a number of independent psychological factors, some of which may be common to several abilities, and some of which may only occur in one ability" (p. 117). This is an improvement upon anything hitherto. But there are several weaknesses in it. His supposed 'independent psychological factors' are gratuitous assumptions; and it is not sufficient to classify tests with the same factors as tests of one and the same ability.

To make a beginning with this matter of seeing just what abilities should be in factor analysis, I propose to define them in terms of non-fractional factors.

V. FACTORS

(1) *Non-fractional factors*

We have to distinguish between two kinds of factors and factor methods, fractional and non-fractional. The idea is very simple. Verbal tests require two factors, g and v, to imitate their intercorrelations. These I call fractional factors (f). But the correlations for verbal tests amongst themselves may require only one common factor V to represent them. This is, in a sense, the prior whole, that is divisible into two fractional parts g and v. Whole factors such as V are called non-fractional factors (nf). They are usually correlated with other nf factors, and have no analytical pretensions. Fractional factors, on the other hand, are usually uncorrelated and analytical in purpose.

To the extent that more than *one* factor is involved in representing a score, the factors are necessarily fractional. But one of these may be an error factor (e), or specific (s). I define a non-fractional factor to begin with in terms of two criteria—*if a set of variables satisfy specified psychological conditions, and can be represented by a common factor D and specific factors* (s), *D is by definition a non-fractional factor* (nf). Relative to other variables, however, the nf factor may be divided into additive or other components, and any such will be by definition a fractional factor (f). For convenience f factors are given small letters, and nf factors capitals.

This distinction between nf and f factors may seem very innocuous. Yet the two have very different parts to play in psychometry. The former, for example, define abilities and types for us, whereas the latter do not (5).

Any non-fractional factor, for cognitive activity, will be an ability by definition. nf factors merely represent unanalysed abilities. Fractional factors, unless they are at the same time nf factors, in no way represent abilities—they *describe* them, or might even *explain* them, but do not *represent* them. Just as in Q-technique I seek to draw sharp distinctions between analytical and non-analytical procedures, so also here in r-technique everything is to be gained by refusing to confuse the direct representation of abilities with their fractional analysis into additive components. The methods used for analysis of correlations into nf and f factors are different (see (5), p. 17).

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(2) *Relation between nf and f factors*

There is a serious objection to Alexander's 'functional abilities'. Two verbal intelligence tests, for example, may have the following factor loadings:

	g	v
Test 1	0·80	0·20
Test 2	0·20	0·80

These would not pass muster either mathematically or psychologically as tests of the same ability. Tests with the same f factor composition only satisfy the tetrad criterion amongst themselves when certain relations exist between the factor saturations.

Thus, if four tests $x_1x_2x_3x_4$ have the same fractional factors α and β,

$$r_{x_1x_2}=\alpha_1\alpha_2+\beta_1\beta_2,$$

$$r_{x_1x_3}=\alpha_1\alpha_3+\beta_1\beta_3,$$

....

and similarly for $r_{x_1x_4}$, $r_{x_2x_3}$, $r_{x_2x_4}$, $r_{x_3x_4}$.

If we now form the tetrad criterion t_{1234},

$$\begin{aligned} t_{1234}&=r_{x_1x_2}\cdot r_{x_3x_4}-r_{x_1x_3}\cdot r_{x_2x_4}\\ &=(\alpha_2\beta_3-\alpha_3\beta_2)\,(\alpha_1\beta_4-\alpha_4\beta_1).\end{aligned}$$

Similarly,

$$\begin{aligned} t_{1243}&=r_{x_1x_2}\cdot r_{x_3x_4}-r_{x_1x_4}\cdot r_{x_2x_3}\\ &=(\alpha_2\beta_4-\alpha_4\beta_2)\,(\alpha_1\beta_3-\alpha_3\beta_1).\end{aligned}$$

Similarly,

$$\begin{aligned} t_{1342}&=r_{x_1x_3}\cdot r_{x_2x_4}-r_{x_1x_4}\cdot r_{x_2x_3}\\ &=(\alpha_3\beta_4-\alpha_4\beta_3)\,(\alpha_1\beta_2-\alpha_2\beta_1).\end{aligned}$$

These tetrads are therefore equal to 0 when

$$\left.\begin{aligned} \alpha_2\beta_3-\alpha_3\beta_2&=0,\\ \alpha_1\beta_4-\alpha_4\beta_1&=0,\\ \alpha_2\beta_4-\alpha_4\beta_2&=0,\\ \alpha_1\beta_3-\alpha_3\beta_1&=0,\\ \alpha_3\beta_4-\alpha_4\beta_3&=0,\\ \alpha_1\beta_2-\alpha_2\beta_1&=0.\end{aligned}\right\}\qquad\ldots\ldots(1)$$[1]

In practice we could inspect a table of f factor saturations, and select from those with the same factor composition those whose saturations are in proportion. When tests involve two or more fractional factors, the proportionality such as (1) expresses is essential for any consistent definition of prior abilities in terms of fractional components.

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[1] See Burt (11).

VI. CONCLUSION

We can now return to consider Table I. We are faced with an unlimited number of mental tests, actual or possible. Systematic test agglomeration makes for comprehensiveness, and would do far more than anything else to place factor work on a sound footing. I have indicated in Table I how this is pursued in Spearman work, in so far as I have been concerned with this work.

The agglomeration also ensures a certain tangibility to f factor components, and suggests where it is, and where it is not, profitable to try to subdivide specific factors into f factors and error components.

But in practice we have not only to analyse tests; we have to define abilities. A number of tests can have the same factor components but different proportions of loadings. Unless the components are in proportion it is difficult on mathematical no less than on psychological grounds to regard the tests as tests of one and the same ability. One suggestion is that instead of dealing with, for example, verbal tests of many different proportions of factors g and v, we could concern ourselves first with those, say, of two proportions, e.g. (*a*) tests with g and v loadings in proportion approximately 4 : 1, and (*b*) others in proportion 1 : 4. These would be defined as tests of two abilities (*a*) and (*b*) respectively. This is what I meant by 'defining' abilities in terms of prior factor analysis.

On the other hand, if thorough test agglomeration is pursued the result would be that tests at any particular level of agglomeration would be analysable into components whose saturations would tend to be in proportion. And this I have indicated in Table I by using capital letters for the 'whole' factors at each level of agglomeration: either in fact, or by selection, the original factors in each column of Table I would be non-fractional, and could be defined as abilities.

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THE FACTORIAL ANALYSIS OF ABILITY

AGREEMENT AND DISAGREEMENT IN FACTOR ANALYSIS: A SUMMING UP[1]

By GODFREY H. THOMSON

I WOULD like to express my intention to strive for impartiality in this summing up. And with that in view I remind myself first of all that neither Thurstone nor any keen follower of his has been present to defend the idea of 'Simple Structure'. Let me therefore for one paragraph hold a temporary brief on his behalf.

I recently heard Prof. Dirac uphold, before the Royal Society of Edinburgh, the thesis that when a mathematical physicist finds a mathematical solution or theorem which is particularly beautiful—and every mathematician knows what mathematical beauty is—he can have considerable confidence that it will prove to correspond to something real in physical nature. Something of this same faith seems to me to lie behind Prof. Thurstone's trust in the coincidence of 'Simple Structure' in the matrix of factor loadings with psychological significance in the factors thus defined. This is of course not all. The conditions laid down in 'Simple Structure' are laid down in order to remove the embarrassing number of degrees of freedom which permit the factor axes to be rotated like a Catherine wheel. They clamp the axes at one point (unless very unusual conditions prevail in the battery) if indeed that point can be reached, which is not necessarily the case in every battery. If it cannot actually be reached it can be approached as nearly as possible. And Thurstone also gives, in pp. 71 and 72 of *Primary Mental Abilities*, and perhaps elsewhere, what one might call his common-sense reasons for thinking it plausible that numerous zeros will occur among the loadings. Moreover, we must remember that he has embarked on a campaign of forming augmented sub-batteries round tests chosen from his original 57, to obtain better definition of his factors, and has obtained what one must admit to be rather encouraging results in his study of the perceptual factor (*Psychometrika*, March 1938).

The disagreement between Thurstone and Spearman as to the general factor *g* is perhaps the most important point of controversy, as it is certainly one of the most easily grasped by the non-mathematical psychologist and one of the most disquieting to him. Prof. Spearman has defended the general factor which will always be associated with his own name; but I think (crossing the court now to speak on his side of the case) that he might have done so more fundamentally than he has. For the objections he has raised to Thurstone's work might conceivably all be overcome. A longer time could be given to the tests. Product-moment instead of tetrachoric correlations could be calculated. Error could be reduced by greater care, and by using large and better samples of subjects. Yet still Thurstone might be able—indeed, with a properly chosen battery, I think he almost certainly would be able—to attain 'Simple Structure' among his loadings, and the complaint that a dismembered *g* was merely being submerged in a sea of error would no longer be justified, at any rate as far as the submergence was concerned. Surely the real defence of *g* is simply that it has proved useful. It still remains to be seen whether Thurstone's primary factors will prove equally useful. A jury in this country would probably to-day give a verdict to Spearman; a jury in another place might give a verdict to Thurstone. The larger jury of the future will, I think, decide by noting which system has proved more useful in the hands of the practising psychologist.

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[1] A contribution to the Symposium presented at the Extended General Meeting of the British Psychological Society held at Reading in April 1939.

I myself lean at the moment more towards Spearman's *g* and his later group factors than I do to Thurstone's, since they seem to me more in accord with the ideas of my own Sampling Theory. On that theory *g* is as it were the whole mind, and tests are part of *g*, not *g* part of the tests. And were that mind entirely undifferentiated, structureless, *g* would be the only factor we need. As the complexity of the mind, and the complexity of the upper brain, is organized (partly by the maturing of hereditary bonds, mainly I fancy by education and life) and integrated into 'pools', 'clusters', call them what you will, so additional factors, additional descriptive coefficients, are needed. It seems to me at present wise to retain one coefficient to express the general depth starting from which the integration, the deepening into subpools, has gone on. But I am not sure, and think the wiser course is to await further papers from workers in Thurstone's school. I think it possible that there will come a stage when practical progress will be most facilitated by a firm voluntary agreement among psychologists that certain well-known tests have such and such a factorial composition, are not to be changed, and may be used to fix some of the factorial axes in a battery. The sample of persons would also have to be defined and different factorial compositions might be given for children and adults, for literates and illiterates, etc., etc. I have reason to think that Dr W. P. Alexander would agree with this position.

The debate about the justifiability of negative loadings has been rather inconclusive for two reasons. It has not been fully appreciated, I think, that the objection among certain psychologists to negative loadings is not an objection to one or two such, for a few negatives can be made plausible, as Dr Flugel has shown. It is an objection to having almost exactly half the loadings negative in the factors following the first one, and to the artificiality, which Dr T. V. Moore has pointed out, of explaining even a matrix of exclusively positive correlations by numerous negative loadings. Prof. Burt's defence of negative loadings and of *bipolar* factors is an out-and-out one. He cannot have his reciprocity principle otherwise. His first factor is the average. Reciprocity only comes in among the remaining factors, and it is essential that they be all bipolar, I think, for this to occur. Burt is in fact defending the use of the principal axes of an ellipsoidal space of $n-1$ dimensions, and in any finite battery (whatever may be the case in a battery of *all* tests) will have to meet the criticisms brought against principal axes by Thurstone in *The Vectors of Mind* and elsewhere. There are also certain mathematical peculiarities in Dr Burt's matrix of bipolar marks which must, I think, be further scrutinized. Some of the latent-roots in both Burt's covariance matrices are zero. And when persons and tests are equal in number some really remarkable consequences may ensue. But these are too mathematical to be discussed here.

Moreover, although I agree with a sort of non-rigorous use of the reciprocity principle, I must emphasize that it only can be found rigorously in a very special sample of people who are all average in ability, and a very special sample of tests which are all of average difficulty. Prof. Burt will protest that it will hold approximately elsewhere, and I readily agree that it will do so if these conditions are not too flagrantly broken.

But only if covariances, not correlations, are analysed. And here let me say that I entirely approve of analysing covariances. My criticism has not been directed (and here I think I have Dr Stephenson with me) against the use of covariances as such, but against the arbitrary nature of the units we have been so far compelled to employ. I am particularly gratified to find that Prof. Burt, from his different approach, is so much in agreement with the general principle that I was led to enunciate from considerations arising out of the Sampling Theory, that what I have called the richer (or as he says the more complex) tests should have the larger variance and that what Spearman calls g-saturation is a tentative measure of their standard deviations if they are in a g-battery. I hope we may co-operate in developing this idea.

Dr Stephenson's exposition of what he calls *non-fractional* factors—compounded in fixed proportions of the 'fractional' factors already known to us—is interesting, but too new to be safely judged yet. It would appear, as he says, to have some affinity with the ideas expounded in Dr Tryon's new monograph on 'Cluster Analysis'. But the connexion of these ideas with the main lines of this discussion are not so obvious as to permit of ready discussion here.

Prof. Burt expressed the hope that the 'tetrad difference' of the four symposiasts would be found to vanish! I think we can all agree that some of the 'disturbers' of unanimity have proved not to be 'significant' and that those which remain have been more clearly defined, the first step towards their reduction. We must all be grateful to the organizers of this symposium for these results.

SOME FACTORS IN THE GENESIS OF INTEREST IN PSYCHOLOGY

By R. C. OLDFIELD

From the Cambridge Psychological Laboratory

I. *Introduction* (pp. 109–110).
II. *Interests and interest-tendencies* (pp. 110–113).
III. *Scientific interests* (pp. 114–116).
IV. *Philosophical interests* (pp. 116–117).
V. *Religious interests* (pp. 117–119).
VI. *Divided interests, vacillation, and apparently contingent decision* (pp. 119–123).
References (p. 123).

I. INTRODUCTION

"BEKANNTLICH gibt es Menschen, denen durch eine sei es angeborene, sei es durch äussere Erziehung erweckte Anlage, ihr kunftiger Beruf von früher Jugend an vor Augen steht. Das sind die Glücklichen, die unbekümmert ihre Strasse wandern können, auch wenn sie sonst mancherlei Hindernissen begegnen....Ich gehöre nicht zu diesen Glücklichen." Thus Wundt ((1), pp. 57–8) and the list of the unlucky includes other psychologists beside him. These others, like Wundt, were not only unable at an early age to experience a sense of vocational certainty, but suffered indecision and change prolonged beyond their natural and customary term.

Conflict in vocational choice may arise in the clash of circumstance with interest. The keenest-minded and the most able may be compelled to turn newspaper boy. William James, born of reasonably well-to-do parents, at one time considered the respective merits of careers in printing and science.[1] But another source of conflict lies within the interests themselves. One interest will urge one kind of activity, direct attention upon one sort of material, induce one variety of cognitive attitude and act. Another demands activity, materials, and cognitive attitudes and acts that contradict or are incompatible with these. Psychologists seem not infrequently to have been the subject of such conflict. Their psychological interests appear often to have arisen in the course of its resolution. And so, perhaps, we may hope to discern, in the division and shift of attention, in the conflict and its resolution, the steps by which a strong interest may develop. We may expect to see the process more clearly displayed here than in the lives of those whom Wundt calls lucky, in whom decision takes place unseen, stirring up no tell-tale agitation of consciousness. To notice instances of divided interest in the earlier lives of those in whom strong psychological interests later developed, and to attempt to relate the reconciliation of these opposing tendencies with the arousal of an interest in psychological problems, will at least provide entertainment for the historically minded.[1] It may in addition supply material for considerations relevant to the general problems of interest-growth.

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[1] See Perry, R. B. ((2), p. 215). The same letter, which is dated 1863, is printed in *Letters of William James* ((3), p. 50).

II. INTERESTS AND INTEREST-TENDENCIES

A certain adjustment of terminology[2] will assist our argument. A prevailing tendency in this field would have us define and name psychological functions in terms of the material upon which they are directed. There results a language scarcely adequate for discussion of the dynamic aspects of interest-formation and interaction. For such a procedure assumes that an interest in a specific type of material is a specific psychological function or entity. Relations between interests *qua* psychological functions can, on this basis, be referred to only in terms of relations between the types of material concerned; similarity and difference between interests can derive only from similarities and differences of types of psychological material.

Such language as this, it is fairly plain, is inadequate to the description of the phenomena as we observe them. In the first place, we frequently wish to express the fact that two individuals may exhibit different kinds of interest in the same type of material. Secondly, we often recognize a degree of persistence and continuity, beneath the surface play of interests, that defies expression in terms of the similarity of material to which attention is successively directed.

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[1] As to material, I have relied on (*a*) autobiographies, (*b*) *History of Psychology in Autobiography* (4), and for those individuals, records of whom exist in neither of these forms, mainly on (*c*) Boring's *History of Experimental Psychology* (5).

[2] The remarks that follow are, it will be evident, largely inspired by Prof. Bartlett's views upon interests, and his notion of the 'fitness' of psychological material to psychological functions. For his discussion of these questions *vide Remembering* (6), pp. 113–14, 210–12, 230–3, 236–7, 290–1, 302, 307). There are, however, important respects in which Bartlett's views, and the field of application which he intends to give them, differ from those presented here, particularly in the relation of *function* to *material*; and I must take all responsibility for my own presentation. I should add that as my own remarks are intended to provide a background to the main topic of the paper, rather than a theory of interests in general, dogmatic brevity seemed preferable to exhaustive treatment. In particular, I would not wish ultimately to maintain that interest-tendencies are to be defined solely in terms of types of operation, while, on the other hand, it is evident that the description of material frequently demands reference to operations.

No exception can be taken to the use of the term 'interest' to denote a behavioural fact that an individual tends to pay attention to, and to behave[1] in a characteristic manner towards, a certain type of material. But such use can give little warrant for the assumption of psychological tendencies structured and differentiated in accordance with the structure and differentiation of the material towards which the individual so behaves. Accordingly, I shall try to use the word 'interest' only in its purely behavioural sense. For those psychological functions which, operating, produce manifestations of interests, I will use the term 'interest-tendency'.

For the sake of brevity I will characterize 'interest-tendencies' dogmatically in the following terms. They are essentially tendencies *to perform certain kinds of operation* upon psychological material—*to treat material in a certain way*. They are not primarily tendencies to pay attention to any particular kind of material. They produce interest in a particular kind of material because that material is suited to the performance of a particular kind of operation. They may be said to be active in the sense that they reach out for material upon which to operate, choosing that which is suited to them—sometimes, when fitting material lacks, they will mould what stands at hand to fit. They must be supposed to arise early, and to persist unchanged, over long periods in an individual's life. Two or more interest-tendencies may conflict at a given time in the sense either that the types of material upon which they are respectively operating are grossly discordant, implying conflict of a logical or moral nature, or that the activity that each compels is materially or logically incompatible with that compelled by the others. They may accord, or become reconciled, in the sense either that discordant behaviour is not produced, or that the behaviour compelled by each is relegated to separate phases of the individual's life, or, thirdly, that material is found upon which all may operate together.

Accordingly, then, an individual will be supposed to possess constitutional propensities to treat psychological material in a particular way, to perform a particular kind of operation whose nature is in a sense independent of the material upon which it is performed (but see footnote 2 to

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[1] The word 'behave' is to be taken to include implicit behaviour.

p. 110). He may possess a number of these propensities various in strength. The environment provides him with a choice of material, and the special circumstances of his life may limit that choice. Propensities, seizing upon material suited to themselves, produce interests. Later, the environmental circumstances may change, the choice of material widen. Material more suited to a particular interest-tendency may offer itself, and a change of interest will result, sometimes dramatic and surprising in the lack of similarity between the two types of material concerned. In many people one interest-tendency is dominant, in others two or more are sufficiently well balanced in strength to induce a conflict of activities. The resolution of such conflict may in certain cases be brought about by the discovery of a type of material suited to the operation of all the interest-tendencies. More generally it may be suggested that the final crystallization of an individual's interests represents the attainment of an optimum equilibrium of the interest-tendencies, in which each obtains the greatest scope possible, having regard to its relative strength, and to the material available.

How then are we to define and name the interest-tendencies? Presumably by the names of the operations, of the ways of treating psychological material, that are characteristic of them. But how are the operations to be defined? In this connexion we must point out that such postulation of determining factors by no means requires that we forthwith define, name and distinguish all the factors postulated. Our procedure will be justified in so far as we are able to characterize, as a first approximation, *any* differentiation within the totality of factors which manifests itself in behavioural consequences. In the second place, the assertion of an underlying differentiation in regard to a limited field of interests, in terms that can apply only to that limited field, carries no implication that the same differentiation fails to hold in regard to other fields, and at other levels. We are concerned here with what, for lack of less invidious terms, may be called the 'higher' or more abstract interests. But it may be the case that, for example, the interest-tendency that determines in part an individual's interest in philosophy may also determine in part his interest in model engineering.

Accordingly I will postulate, for our present purposes, two specific interest-tendencies, the *Empiric* and the *Systemic*. The empiric we may provisionally define as a propensity to treat material in its aspect of relating to experimental fact, the systemic as a propensity to treat material in its aspect of possessing internal structure.

Lastly, we require some means of detecting the operation of these tendencies. Here we must distinguish between the means that might be adopted in a detailed observational study of the development of an individual, and those we must fall back upon where our material consists in written matter of varied fullness and documentary value. In the former case it would be possible to record not only the material upon which an individual's attention is directed, but also the nature of his activity in regard to that material. It might be possible to see, for example, that while a dominant interest is economics, he is concerned more with the erection of general theories for the explanation of general conditions, than with the determination of empirical fact, suggesting the operation of the systemic rather than the empiric tendency in the arousal and maintenance of his interest.

But here we are driven back upon the argument from the existence of interests to the operation of interest-tendencies. We have seen that to a given interest-tendency some kinds of material are more fitted than others, and that interest-tendencies tend to seize upon material most suited to them. The existence of particular interests, therefore, affords presumption of the operation of particular interest-tendencies, *in circumstances where the range of material at the disposal of the individual is wide enough to allow the factor of 'choice of the fittest' sufficient scope.* Where the range of material is very narrow, as at an early age it may well be, any material, although fundamentally unsuited, may be seized upon and an interest developed. And in such a case the argument from interest to interest-tendency would be highly unreliable.

But in the cases we shall consider this condition is, in general, sufficiently fulfilled. It may be asked why, for the present purposes, we have devoted some pages of discussion to the erection of the concept of interest-tendencies, when admittedly the only means of detecting them in the material under consideration is through the occurrence of interests. We may reply that we shall have occasion to notice the arousal of psychological interests apparently as an outcome of a previous conflict or division of other interests. Now it is difficult to imagine that an interest in psychology can *causally be produced* by the conflict of interests in physics and philosophy. But it is possible to see interests in physics and philosophy as manifestations of two interest-tendencies which, by both seizing upon the material of psychology, produce an interest in psychology, and bring about a resolution of the previous conflict.

III. SCIENTIFIC INTERESTS

Many who have contributed much to psychology must be regarded, in the light of the whole of their achievements, primarily as workers in other fields of science.[1] In such cases there can be little significance for our purpose in tracing the growth of scientific interests which in fact dominated the individual's life. We shall be concerned only to illustrate the fact that *among psychologists proper*, and even among those whose contributions to the subject have been mainly of a philosophical nature, there is a somewhat surprising prevalence of scientific interests.

Bain, up to the time of his entrance into college, was a self-educated man, devoting himself almost exclusively to mathematics and physics, and attaining considerable proficiency. Even while pursuing the multifarious courses required for the Scottish University degree of those days, and at a time when very strong philosophical interests had arisen, he found time to study other scientific subjects, including anatomy (7). Fechner, matriculating as a medical student, turned to physics, and became a professor in this subject, publishing papers of some importance upon the measurement of the E.M.F. of cells. The passage of his interests from physics to psychology was marked in part by publications upon colour sensations and after-images, and in part by a crisis during which he became convinced of a philosophical mission. The desire to set his philosophy upon an empirical basis resulted in the psycho-physical work for which he is remembered ((5), pp. 265–74). Lloyd-Morgan's formal college education was that of a mining engineer ((4), II, pp. 237–64). Drever, in the course of a lengthy education, seems to have studied almost every branch of science, and at one time decided upon a purely scientific career ((4), II, pp. 17–34). James's more academic youthful preoccupations and vocational intentions were biological. He was a member of Agassiz's South-American scientific expedition in the capacity of naturalist. At various junctures he intended to make scientific research and teaching his career. He was medically qualified, and obtained his first academic post as lecturer in physiology at Harvard.[2] McDougall had an extensive scientific education, and confesses that he found difficulty in choosing a branch of science whose problems should match his capacities ((4), I, pp. 191–223). C. S. Myers, whose Cambridge education was scientific and medical, was at one time nearly drawn to pathological research as a vocation ((4), III, pp. 215–30). Ward, the purest of the pure in his later writings, sowed a few youthful wild oats in natural science (8), publishing on the nervous system of the crayfish, and upon the vitality of the common snail (9). Lotze's scientific interests were sufficient to allow him to select his father's profession of medicine, although even during the time of medical training he was "...by temperament...more disposed towards the arts and philosophy than towards science and medical practice" ((5), p. 251).

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[1] E.g. Helmholtz, Galton, Hering, Loeb. But I have not, in selecting material, had too strict regard to a proper definition of the term 'psychologist'.

[2] James's life constitutes perhaps the most striking illustration of the points raised in this paper. Besides the *Letters* (3), we now have the magnificent source provided by Perry (2) in which a mass of previously unpublished letters are nicely blended with wise narrative.

Even among those who never seem to have contemplated a career in one of the well-established natural sciences, there are a number who went out of their way in student and early post-graduate days to study science. Fröbes, while immersed in the exacting educational programme of the Society of Jesus, a programme in which dogmatic theology and its contributory philosophy predominate, pursued the physical sciences with enthusiasm ((4), III, pp. 121–52). Titchener, having read "Greats" at Oxford, promptly secured a research studentship in physiology, later writing on biological topics.[1] Stanley Hall, even as late as his second visit to Germany, mixed in a fair proportion of science among the numerous courses on almost every kind of subject that he attended, and found time for experimental work in neurology with von Kries ((12), (13), p. vii).

A factor which quickened the scientific interests of philosophical students round about the second half of the last century, particularly in Germany, was a prevailing tendency to regard science as essentially the true method of philosophy (see (5), p. 352). In consequence we find Stumpf, instigated by Brentano, undertaking "...to learn science in the chemical laboratory, even to the extent of setting the building on fire..." ((5), p. 352), while Woodworth, less disastrously (at least so far as his personal confessions indicate), pursued a similar course with the same motive, later hesitating between psychology and mathematics as a career, and doing a year's research in the physiological laboratory at Harvard, before settling down as a psychologist ((4), II, pp. 359–80). The experimental work for which Fechner and Ebbinghaus are remembered originated in part in the same tendency.

Strong interest in mathematics appears to be a less common occurrence. In only one or two cases is interest so strong, or ability so great, that the individual considered finding an academic vocation in this subject. Woodworth and Yerkes are exceptions.

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[1] One may perhaps be permitted to express the sincere hope that Prof. Boring's next achievement will be a *Life* of Titchener. For meagre, but appetizing accounts, *vide* Boring ((5), pp. 402–13); Boring (10) and Warren, H. C. (11).

It is clear, then, that scientific preoccupations played an important, and sometimes a dominant part in the development of interests in a number of those who finally turned to psychology. Frequently these preoccupations induced the individual to decide upon a career in science, or caused him to undertake scientific research. And they are to be found even among those, like James, Lotze and Ward, whose later work tended more and more towards the purely philosophical.

In a number of the instances we have noticed, it would seem reasonable to interpret the existence of scientific interests often co-existing with philosophical as the manifestation of an empiric interest-tendency. For it is often possible, without straining the argument too far, to see the later operation of the same tendency even where pursuits other than the purely scientific have become dominant. James's philosophy is shot through and through with evidence that, even in the field he finally chose, a strong empiric tendency found the means of expression. Fechner, in the moment when a fervent, almost mystical, sense of having a philosophical mission seized him, sought an empirical basis for his system. Titchener's empiricism underlies his structuralism and his passionate rejection of impalpable cause-factors. It would scarcely be too much to say that Lloyd-Morgan's Canon was forged from ore of his own mining.

IV. PHILOSOPHICAL INTERESTS

It is not in itself a matter for surprise that, with very few exceptions, the biographical material makes mention of more or less strong philosophical interests at one period or another in the lives of the individuals. Those about whom information is available belong to older generations. In the times of their youth psychology was approached through, and regarded by many as, a branch of philosophy. Again, while it is in the very nature of natural science, by one device or another, to eliminate psychological issues from its content, philosophy has been less successful in this respect, and the pursuit of metaphysics suggests problems of psychology at every turn. Accordingly, the instances of philosophical preoccupations which may be considered significant from our point of view, are those in which they accompany other, generally scientific, activities, and other educational programmes and vocational intentions.

Binet was strongly attracted towards philosophy while pursuing the study of medicine (14). Janet, effecting a compromise between strong scientific and theological tendencies, turned to philosophy and became a *lycée* professor in that subject at the age of 22 ((4), I, pp. 123–33). Lloyd-Morgan, while applying himself to the vocational training of a mining engineer, "...kept close, though necessarily occasional, touch with Berkeley and Co...." ((4), II, p. 240). G. E. Müller studied history because he intended to teach it as a profession, and philosophy because he liked it ((5), p. 361). C. S. Myers acquired philosophical interests while an undergraduate studying science and medicine ((4), III, p. 216), and later published papers on purely philosophical topics. Spearman confesses to "...an excessive but secret devotion to philosophy..." before and during his school days, a devotion which survived an army career ((4), I, pp. 299–333). Lotze's philosophical interests are said to have arisen at an early age, and he continued to pursue them throughout a brief medical career ((5), pp. 250–4). James's numerous preoccupations as a young man included philosophy. The Agassiz Expedition interrupted his scientific and medical studies at Harvard, and in letters from Brazil, "behind his more or less playful manner there is unmistakeable evidence of a brooding preoccupation with philosophy" ((2), p. 215). He "...contributed little to the work of the expedition, but he made in Brazil one important discovery: he found that he was a philosopher" ((5), p. 495).

An interest in philosophy, then, is not uncommon even among those psychologists whose original vocation ranged from soldiering to medicine and mining. And it is reasonable to interpret the maintenance of such interests, concurrently with other pursuits, as evidence of the operation of some general systemic interest-tendency.

V. RELIGIOUS INTERESTS

The fact that mention is made of religious factors in a large number of cases where biographical material is available is not surprising.[1] In many instances the reference relates to a religious family or community background, and the same might be found in any other set of comparable records of intellectual individuals in the latter half of the nineteenth century. It is interesting to notice, however, that some individuals attribute their psychological interests to earlier religious preoccupations.

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[1] Mention occurs in about half the total cases. Thoughts of entering the ministry seem to have been entertained by only ten of the individuals concerned, and of these three only reached the stage of ordination. Two of these subsequently seceded from the Church, the remaining one being Frobes. Brentano's ecclesiastical career was attended with results more stimulating to the Church than satisfactory to himself, and Ward's incumbency of a Nonconformist Chapel was brief. In three cases, McDougall, Brentano and Stumpf, there is definite mention of a religious crisis.

Höffding states that he was led "...partly through the problems of religious emotions to speculate on the mind of man" ((4), II, p. 197). Judd writes: "I have no doubt that my early intensive religious training had a large influence in directing my later interests towards a study of man and his mental life" ((4), II, p. 207). Janet is of the opinion that his interest in psychology and philosophy was the result of a conflict between two tendencies, the one religious and quasi-mystical, the other scientific ((4), I, p. 123). Baldwin writes that his "...interest was directed towards psychology through his early intention to go into the ministry" ((4), I, p. 1). Freud declares: "My early familiarity with the Bible story (at a time almost before I had learnt the art of reading) had, as I recognized much later, an enduring effect upon the direction of my interest" ((15), p. 13).

What part can we suppose religious preoccupations to play in the later development of psychological interests? There can be no denial of the fact that the content of religious knowledge and experience may in itself suggest problems of psychological import. But it may be that this fact does not exhaust the possible connexions between religion and psychology. Religious teaching frequently provides for the growing child the first body of more or less coherent psychological material. It offers admirable opportunity for the working of the systemic interest-tendency, as any casual study of children's theological systems will show. I will suggest that early religious interests afford evidence of the operation of the systemic interest-tendency, later to find a nicer balance with the empiric in becoming directed upon the problems of 'man and his mental life'.[1] In this suggestion there is, of course, no implication that all early religious interests are the product of the systemic tendency, or that it alone can ever be effective in their production.

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[1] A problem perhaps relevant is that of the significance of experiences and phantasies, apparently not uncommon in intelligent children, incorporating religious, mystical; and astronomical or cosmological elements. The experience may consist merely in a tendency to preoccupation concerning the ultimate significance of the universe. Another variety is described vividly by Klemm ((4), III, p. 155) and is worth quoting. "The first connected realm of facts that presented itself to me was the kingdom of the stars. The boy's fantastic thought, kindled no less by the infinity of space and time than by the unattainable and supramundane, rose upward through the silent winter nights. The contradictions of religious dogma were challenged and experienced over and over again with a peculiarly sweet tension. When I was 15 I received a gift of a three-inch spyglass with a lens of × 80 magnifying power, and with the help of wood and paper constructed a telescope." The combination of an almost mystical experience with a truly empirical curiosity, seems to be characteristic. Another instance is provided by the highly systematic fantasies of voyages through the solar system developed by Wirth and his brother (16). To pursue the matter further here is impossible, but one is tempted to suggest that such phantasies may represent the operation of active interest-tendencies upon such material as lies at hand, material which is not truly fitting.

VI. DIVIDED INTERESTS, VACILLATION, AND APPARENTLY CONTINGENT DECISION

We come now to an aspect of the subject which can scarcely fail to strike forcibly anyone who studies the biographical material, however superficially. It reveals in a surprising degree the prevalence of conflict of interests among psychologists. Such conflicts are manifest in a division of strong interests often persisting beyond the phase of individual development in which they are ordinarily considered normal. They are shown most clearly in a tendency to vacillate between the adoption of different intellectual pursuits, and in some cases are indicated, one may suspect, in the apparently contingent nature of the particular event that leads to a decision.

Division of strong interest, and a tendency to vacillate between different intellectual pursuits, are apparent in many cases. Külpe intended to study history in the first place, but going to Leipzig for this purpose, he came into contact with Wundt, and was for a time diverted to experimental psychology. He then went to Berlin where he read history with Mommsen. This phase lasted for a single semester, at the end of which he returned to psychology, this time with G. E. Müller at Göttingen. However, still undecided as to whether History or Psychology should be his life-work, he repaired to Dorpat, where he again read history. In 1887 he took his degree in psychology, finally deciding in favour of this subject ((5), pp. 386–7). In his later years he turned to philosophy.

G. E. Müller's primary interest at the age of 15 was philosophy, and at this time, he tells us, he was inclined to mysticism from which he was rescued by the writings of Lessing. As a student he read history for the sake of a teaching career, and philosophy because of a strong interest. After a period of history, archaeology and the History of Art, perusal of Lotze's writings caused him to study philosophy as *Hauptfach*, and a considerable conflict between historical and philosophical interests ensued, aggravated by the belief of the times that the scientific method, in which he had had no training, is the true method of philosophy. The conflict seems to have been painful, but was resolved by the outbreak of the Franco-Prussian war. After a year of service in the field Müller found history a 'narrow' subject, and turned to the study of science ((5), pp. 361–2).

Wundt's decision to specialize in the study of physiology was the result of painful reflections upon the manner in which he had dissipated his efforts during his first student year, coupled with the courage conferred by the beauty of a boat trip up the Neckar ((1), pp. 71–2). Happily for experimental psychology this decision did not too rigidly determine his subsequent interests.

Such instances of conflict and vacillation are interesting in themselves. But more significant is the question of the manner in which they issue, and are resolved. The resolution, when it comes at all in definite fashion, may happen by one of three more or less distinct means. In some cases an event apparently contingent, and irrelevant to the real issue, may seem to bring about a decision. In others, where there are two or more conflicting interests, it may prove possible in the ultimate event for each to be separately pursued. Lastly there may come a solution through the discovery that a new interest—in the cases we are considering generally an interest in psychology—offers a compromise and reconciliation, allowing the tendencies harmonious expression in unified activity upon a single field of material.

Personal influence may operate in conjunction with an event apparently so contingent and irrelevant that one can scarcely help inferring a pre-existing instability of interest.

Carr, having decided to major in mathematics, arrived at Colorado University, and found that he disliked at sight the instructor in his chosen subject. Happening to meet the professor of psychology however, and finding him genial and encouraging, he decided on the spot to turn psychologist, "...although I knew practically nothing about the nature of the subject" ((4), III, pp. 69–72). Seashore, whose main interests during his early college days were mathematics and languages, was inspired by friends to go to Yale in order to study philosophy. He happened, however, to arrive there on the day the psychological laboratory was opened, and, on the advice of Ladd, he entered the course, although he too knew nothing of it previous to his arrival ((4), I, pp. 225–97). Stern, intending to study philosophy and philology, soon realized that "...the petty pursuits of philology could not thrill me". A deep impression was made upon him by Ebbinghaus, whose point of view appealed to Stern's love of the empirical. Stern had, he says, "...a strong desire for exact and detailed work, and direct contact with the concreteness of fact". Philosophical speculation, however, continued to be an interest, although he regarded it as a weakness ((4), I, pp. 335–88). Dodge started life as a philosopher, but in Erdmann's seminar "...the coincidence of a...humiliating experience and an unprecedented piece of good fortune diverted me into experimental psychology". The first of these events consisted in Dodge missing a rather obvious point in connexion with Kant, upon which Erdmann remarked: "Herr Dodge, I fear you will never make a philosopher". The second was Dodge's success in inventing for Erdmann a tachistoscope of elaborate requirements, a technical triumph which Erdmann had been assured by his physicist friends was impossible ((4), I, pp. 99–121).

Solution through the continued but separate pursuit of the conflicting interests is not for our purpose of any great psychological interest. It is sufficient to note that circumstances may allow of it, the second interest-tendency obtaining outlet in hobbies and spare-time occupations. But the type of solution in which incompatible tendencies find reconciliation by becoming directed upon some new topic appears commonly as the means by which individuals are led to the pursuit of psychology.

Janet speaks of the general direction of his philosophical and psychological interests as being determined by a sort of conflict. One set of tendencies towards natural science, botany in particular, amounted to a passion which "...determined my taste for dissection, precise observation and classification". Another tendency, never wholly satisfied, was religious, and Janet confesses that his mystical impulses were never wholly controlled. Between the two opposing forces an interest in philosophy arose as a compromise ((4), I, p. 123). It may be supposed that the empiric tendency found insufficient outlet in philosophy, and determined the later change in Janet's interests towards psychopathology.

Washburn writes that "At the end of my senior year, I had two dominant intellectual interests, science and philosophy. They seemed to be combined in what I had heard of the wonderful new science of experimental psychology" ((4), II, p. 338). Woodworth, following a period in which he had devoted himself variously to philosophy, mathematics, and science, remarks: "And so...I turned from philosophy, half expecting to continue some effort in it, but discovering as time went on that psychology was an amply sufficient occupation" ((4), II, p. 367).

Fechner, medical student turned professor of physics and humanistic satirist, suffered a crisis of health which left him with a "...deepened religious consciousness" and a philosophical mission. He felt at the same time that philosophy required a scientific basis, and in 1850 "He saw that the thing to be done was to make the relative increase of bodily energy the measure of the increase of the corresponding mental intensity...". This conclusion led to the experimental work forming the basis of the *Elemente der Psychophysik*, which will be remembered when the satires of

'Dr Mises', and the sevenfold call of the philosopher Fechner to a dormant public, are forgotten ((5), pp. 267–70).

Klemm writes that at an early age "...the joy which I had in scientific insight was perturbed by the philosophical deductions especially of the materialistic school of thinkers". At Munich, where he had enrolled in Lipps's psychology course, "One thing struck my attention, the small collection of apparatus for experimental psychology, which stood unused and somewhat dust-covered, in a corner of the seminar room....Here I felt the presence of a gap, through this gap I passed to another great field, that of experimental psychology ((4), III, pp. 153–61).

Stern's philosophical and empirical tendencies found a compromise in psychology. At the age of 19 he wrote in his journal: "In Philosophy I must find my salvation or perish. One consolation there is, namely, that a philosophical discipline is open to me as my special subject, psychology..." ((4), I, p. 340). Stumpf, after a period of development which included strong and persistent interests in music and philosophy, and a religious crisis of the most intense and agonizing kind, finally decided to devote himself "...to that field which, connecting my musical experiences with the interests of psychology, seemed to me, personally, the most promising". In his case, however, there is evidence of a continued clash of empiric and systemic tendencies, for he mentions that, with Aristotle, he finds theory the sweetest of all, but it is always a comfort to pass to observation ((4), I, pp. 396–7).

James's conflicts of interests and difficulties of vocational decision, aggravated by a superbly delicate moral sensibility, are too numerous and complex for brief record to do them justice. One quotation must suffice to illustrate the intensity of the doubts to which he was subject. He writes in 1868, at the age of 26: "I have not got started properly on any line of work yet, but am hovering and dipping about the portals of psychology. The fact is that I am about as little fitted by nature to be a worker in science of any sort as anyone can be, and yet...my only ideal of life is a scientific life." But later in the same letter he decides that "All in all, even the sweepings of morality are better than chemical reactions", and adds, "Thus do I lash my tail and start myself up again..." ((2), I, pp. 286–8).

Many factors may contribute towards the growth of an interest in psychology. It is an interest which, for reasons relating simply to its present academic status, is in any case liable to develop later than are interests, for instance, in philosophy, history or biology. But one means by which it may be reached is, I suggest, through the existence and balance of two generalized tendencies, the one to treat material in an empiric, the other to treat material in a systemic, way. In such a case there may occur a competition of activities and a conflict of interests. The situation then becomes resolved through joint direction of both tendencies upon the material of psychology, which can offer scope for the two together.

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THE EFFECT OF ADAPTATION ON THE DIFFERENTIAL THRESHOLD FOR SOUND INTENSITY

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From the Psychological Laboratory, Cambridge

I. *Introduction* (pp. 124–126).
II. *Apparatus* (pp. 126–129).
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I. INTRODUCTION

THE influence of intense stimulation on the absolute sensitivity of the human ear has been the subject of several previous reports (Rawdon-Smith, 1934, 1935, 1936). In these, it was shown that pure tone stimulation at relatively high loudness levels yielded a transient loss of absolute sensitivity frequently of considerable extent, and exhibiting several interesting features. Thus the extreme lability of the phenomenon and its partially binaural nature (by which is meant the tendency for stimulation of one ear alone to modify the sensitivity of both) led to the supposition that the effect was partially mediated by changes in the central nervous system. Indications of a further peripheral change have, however, also been shown both for the human subject (Rawdon-Smith, 1934) and for the physiological preparation (Rawdon-Smith, 1938, p. 117). The former hypothesis, namely, that of the central locus of certain of the observed phenomena, achieved confirmation in the finding that under certain circumstances it is possible for an unexpected stimulus, even one in an alternative sensory mode, to cause a sharp, though temporary, recovery of normal sensitivity (Rawdon-Smith, 1935, 1936). Since this effect clearly resembles the general phenomenon of 'disinhibition', we have preferred to employ the term 'auditory inhibition' rather than the more conventional 'auditory fatigue', to describe the loss of sensitivity produced by the initial stimulation.

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[1] Senior Student for the Royal Commissioners of the Exhibition of 1851.

In view of these several unexpected properties, it has appeared desirable that our investigation be extended to determine whether or not intense stimuli of the type found to yield so great an effect on the absolute sensitivity might not to some extent influence the differential sensitivity of the human auditory mechanism. The present account, therefore, deals with the results of an investigation of the effect of such stimuli on the differential intensity sensitivity of the ear.

Earlier work on the differential intensity threshold for the human ear includes that of Knudsen (1923) and Riesz (1928). As a source of sound, Knudsen used a thermionic tube audio-oscillator, the output of which passed via a galvanometer and resistance network to a telephone installed in a sound-proof room. A motor driven key-switch operated to short-circuit one of the resistances of the network, and at this point the intensity of the sound in the telephone was increased. The amount of this intensity rise could be altered by adjustment of the value of the short-circuited resistance. Starting with an easily supraliminal change, this value was gradually decreased until the just noticeable difference was found. Unfortunately, however, the introduction of an instantaneous intensity rise in the manner discussed carries with it certain important limitations. When either the absolute intensity or the intensity change or both are large, the arithmetical change of force on the diaphragm at the moment of intensity change is sufficient to produce an audible note of the natural frequency of the diaphragm of the telephone in use. This phenomenon is objectionable, as it produces a readily audible click, when the intensity change is effected. These theoretical suppositions were noted by Ewing & Littler (1935) in a related investigation; and were adequately confirmed during certain work preparatory to an investigation of the variation of the value of the differential intensity threshold, with rate of change of intensity (Rawdon-Smith & Grindley, 1935). If Knudsen's technique is adopted, therefore, as the value of the intensity change is reduced so will the loudness of the click be reduced. A human subject of such an experiment must, therefore, make any judgment as to the perceptibility of the change in question at least partly on the basis of the loudness of the click accompanying the change.

Knudsen's work was repeated by Riesz (1928), using a technique against which these criticisms cannot be levelled. This work is discussed in detail by Fletcher (1929, pp. 147–51) and need not, therefore, be further mentioned here, save to note that the very considerable technical elaboration which Riesz was able to introduce would ordinarily render investigations of this character somewhat beyond the scope of most laboratories. We have, therefore, evolved a simpler technical arrangement, carrying with it the advantages of Riesz', and additionally conferring the benefit of giving a pure intensity modulation, constant in regard to frequency.

II. APPARATUS

A sinusoidal alternating voltage, generated by a beat frequency oscillator, is taken through a calibrated attenuator to a special amplifier. The output from this, fed to a telephone receiver, produces an approximately pure tone. The block diagram (Fig. 1) shows the layout of this equipment.

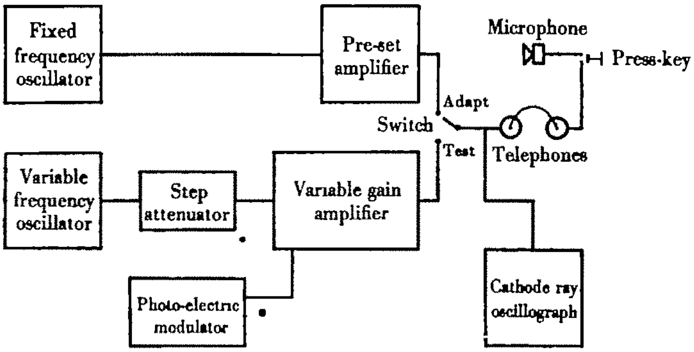

Fig. 1. Block diagram showing layout of apparatus.

The amplifier contains one stage whose amplification can be rhythmically varied by suitable application of a voltage derived from a photoelectrical modulator. It will readily be seen that a rhythmic change in amplification of known amount will produce a tone whose intensity is modulated over a predetermined range.

The design of the modulator is such that no click, or other spurious indication that the loudness of the tone has been altered, is audible in the telephone receiver. It consists essentially of a disk of special shape, rotated in the path of light rays falling on a photoelectric cell in such a way that the amount of light falling on the cell (and consequently its electrical output) is varied in a rhythmic manner. The output voltage so generated is directly coupled in such phase as to decrease the standing negative bias on the suppressor grids of a pair of Mazda ACSP 1 pentodes, operating in biphase as part of the amplifying chain (Fig. 2). The stage gain is consequently increased when more light falls on the photoelectric cell.

For the purpose of the present experiment, a sinusoidal intensity modulation of the tone in the telephone receiver was deemed to be desirable, because such an intensity change scatters least energy into parts of the spectrum remote from the test frequency. This necessitated a sinusoidal change of voltage from the cell, since the amplification of the ACSP 1 valve bears a very nearly linear relation to the applied voltage on the suppressor grid. To obtain this sinusoidal voltage, a cardioid disk (Fig. 3*a*) must be rotated in the path of parallel light rays falling on the photoelectric cell. The arrangement employed (Fig. 3*b*) consists of an optical bench carrying a light source, a condenser, a slit to cut off stray light, and the photoelectric cell itself. These can be moved relatively to one another and to the rotating disk, thus enabling correct adjustment to be found very simply. The disk is carried on a rod, and is rotated by a synchronous motor through a belt reduction drive arranged to give one complete rotation of the disk per second; each full intensity modulation cycle thus lasts 1 sec., this modulation frequency having been found by preliminary experiment to be most suited to the observers.

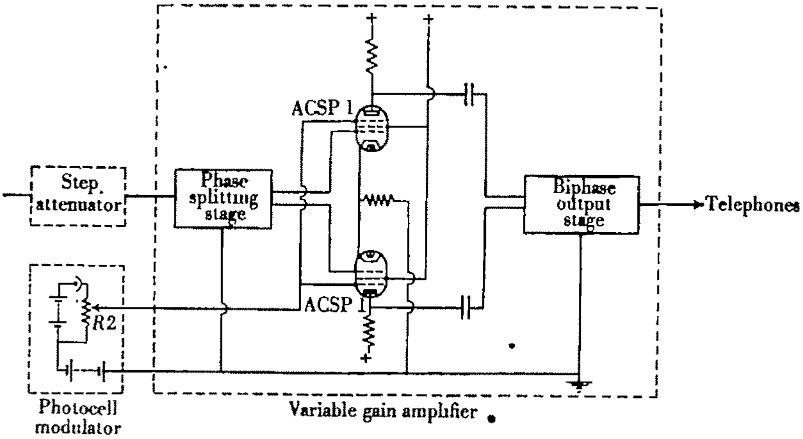

Fig. 2. Circuit diagram showing connexions of photocell modulator and variable gain amplifying stage.

The depth of the modulation is varied by altering the intensity of the light source, and a variable resistance (*R* 1, Fig. 3*b*) in series with the battery supplying the bulb is used for this purpose. The proportion of the output from the cell applied to the grids of the ACSP 1 valves is adjusted on *R* 2 (Fig. 2) until an intensity change subliminal to all subjects occurs with the dimmest light used, and a suitable maximum intensity modulation depth is attained with the full brightness. Varying the intensity of the light thus gives a smooth change in the depth of modulation, and it was this control which was used in taking readings, in the present series of experiments.

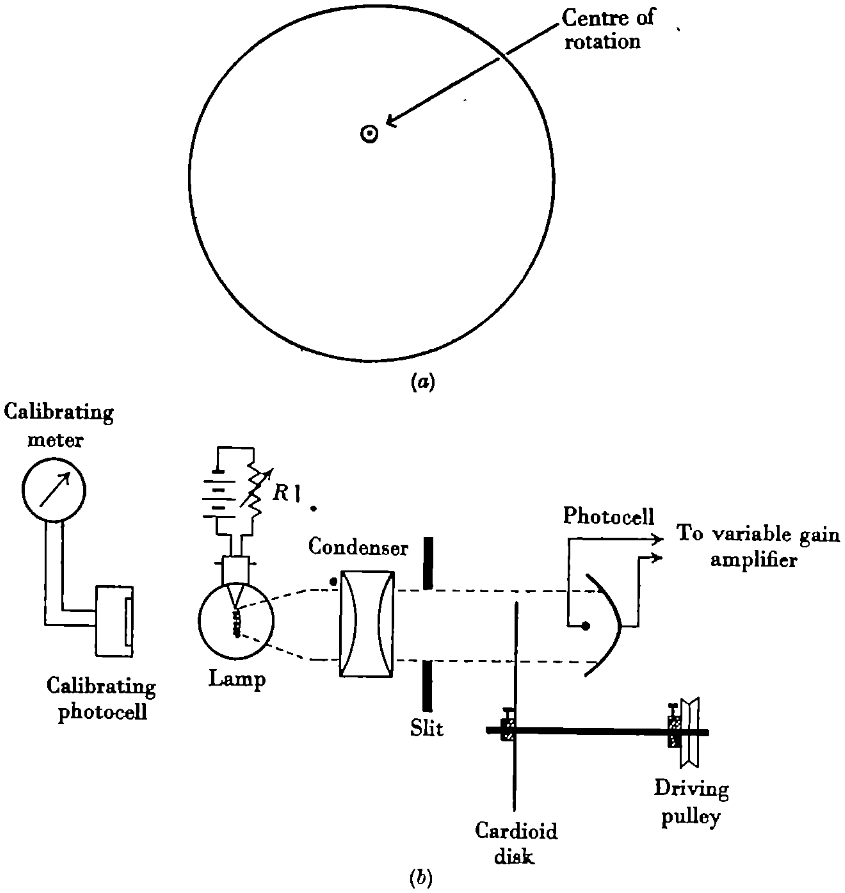

Fig. 3. (*a*) Shape of cardioid disk. (*b*) Arrangement of light modulator: light from the lamp passes through the condenser and slit, and falls on the photocell; the parallel rays are rhythmically interrupted by the rotating cardioid disk. A second, generator type, photocell, and a microammeter, are used to measure the intensity of the light, which is varied by *R* 1.

With the light source at its dimmest, the observer reports no change in the intensity of the sound in the telephones; slowly increasing the intensity of the light increases the depth of modulation, and a point is reached at which the observer reports a rhythmic change in the loudness of the tone. At this point, a reading proportional to light intensity is taken by means of an independent generator type cell and micro-ammeter (Fig. 3*b*) since a record of this is essential for the calibration of the apparatus.

Calibration is accomplished as follows: a number of readings is taken, and the average light intensity at the threshold point determined. The light source is then set at this value, and a rectifier type output voltmeter switched into circuit across the telephone receiver. The output voltage is read with the cardioid disk rotated first so as to give minimum output, and secondly, so as to give maximum output. The ratio of maximum to minimum voltage across the telephones at the differential threshold has thus been determined, and this ratio may be expressed as follows:

$$\text{voltage ratio} = V + \Delta V : V \qquad \ldots\ldots\text{(i)}$$

(where V is the voltage output at minimum, and ΔV is the voltage output at maximum).

The method of calibration outlined above has the advantage that variations in the voltage supply to the light source, ageing of the bulb, etc., are of no consequence, since it is the actual brightness which is noted at each reading, by means of the auxiliary photocell equipment; moreover, since the output voltage is measured immediately after taking readings, long-period changes in the amplification constants are also unimportant.

The output to the telephones is also applied to a cathode ray oscillograph, and during the course of an experiment, careful check is kept on the waveform of the intensity modulation, and on the frequency of the tones employed. The latter is checked by obtaining a Lissajous's figure against a constant frequency source applied to the other deflectors of the cathode-ray tube.

A voltage from a second, fixed frequency, oscillator (Fig. 1) amplified by a pre-set amplifier whose output stage is similar to that of the main amplifier described above, could be switched into the telephone receiver at will, in place of the output from the main amplifier, and the tone so produced was used as the adaptation tone in the present experiments.

III. PROCEDURE

The observer was installed in a sound-proof room, inside which the external noise level was below threshold. He was seated at a table, on which was a silent, mercury-operated key, pressure upon which sounded a buzzer in the experimenter's room. An additional microphone and loud-speaker system enabled comments by the observer to be heard by the experimenter. The observer wore a pair of telephones on an adjustable head band, in one of which he heard the experimental tone, and in the other of which he could hear the experimenter's voice, provided the latter pressed a key in order to speak to him. The telephones were marked, so that the experimental tone was always sounded in the same telephone on the same ear throughout any one series of experiments.

The absolute threshold of hearing was first assessed by ordinary audiometric methods, using the system of attenuators in association with the main amplifier, at the frequency at which the subsequent tests were to be carried out. This determination was essential in order to ensure that the differential judgements could be carried out at an intensity level referable to the observer's own absolute threshold.

This preliminary measurement was accomplished in 2–3 min., and the observer was then warned that he would receive an adapting tone for 2 min., after which he would be switched noiselessly to the test tone, whose intensity modulation, inaudible at first, would gradually be increased until he signalled that he was able to detect the rhythmic intensity change. Immediately a judgement had been given, the adaptation tone was switched on for a further 15 sec., and then another test taken; this procedure was continued until five readings had been taken, to enable a fair average to be made, after which (the adapting tone still being kept constant) a further five readings were taken at each of a number of different test intensities. This occupied one complete session. It was thus possible from the data of a single session to plot the differential threshold at each of a number of test intensity levels ranging from 10 to 110 db. above the absolute threshold, when the ear was adapted to a certain adaptation intensity level.

In all, seven sessions were required of each observer, each one being undertaken at a different adaptation intensity, so that the differential threshold, at stated test intensity levels from 10 to 110 db. above the absolute threshold, was known under conditions of adaptation intensity varying from silence to 110 db. above the absolute threshold.

During the course of a session, the observer was allowed to make any comments he wished and these were heard by the experimenter through the microphone communication channel. It was thus possible for the experimenter to disregard any readings which were acknowledged by the observer at the time to have been unreliable. In a session lasting 20 min. or more, it was almost inevitable that the observer's attention would wander, or that sounds of his movements in breathing would on occasions mask the test tone (more particularly at low intensities). When such events occurred, he communicated with the experimenter, who was thus able to make an appropriate note.

Three observers only were used in the present experiments, owing to the length of time taken in obtaining a full series for each, and to the fact that inexperienced observers, some of whom were tried in an incomplete series, had difficulty in presenting consistent judgements. All three observers employed gave absolute thresholds within 4 db. of each other, and closely corresponding to the normal value for the average human ear at the frequency used.

IV. RESULTS

The voltage ratio of maximum to minimum of the intensity modulation at threshold, obtained as explained above (p. 129), may be treated in either of two ways, as follows:

(*a*) The actual increase in sound intensity necessary to elicit a judgement, expressed in decibels, is simply obtained from formula (ii) below:

$$\text{Differential threshold (decibels)} = 20 \log \frac{V+\Delta V}{V}. \qquad \ldots\ldots\text{(ii)}$$

(*b*) Weber's fraction, $\Delta I/I$, can be obtained from the data in the following way:

$$\text{Voltage ratio} = \frac{V+\Delta V}{V}, \qquad \ldots\ldots\text{(i), (p. 129)}$$

therefore $$\text{Energy ratio} \quad \left(\frac{I+\Delta I}{I}\right) = \frac{(V+\Delta V)^2}{V^2},$$

hence $$\text{Weber's fraction } \frac{\Delta I}{I} = \frac{(V+\Delta V)^2 - V^2}{V^2}$$

$$= \frac{2V\Delta V + \Delta V^2}{V^2}. \qquad \ldots\ldots\text{(iii)}$$

It is convenient to use Weber's fraction as a measure of the differential threshold, and if these values are plotted as the ordinates of a graph (on a logarithmic scale) the abscissae being the intensity levels of the test tone in decibels above the absolute threshold, graphs may be drawn showing the differential threshold at various test intensities for a given intensity of adaptation tone.

A typical graph (Fig. 4) of this type, plotted from the results of a single session by one of the observers for an adapting tone at an intensity level of 70 db. above the absolute threshold, shows the general characteristic of the differential threshold. It will be seen that the curve corresponds to the generally accepted shape of the Weber's law curve when $\Delta I/I$ is plotted against I for any sensory mode, but it is of interest to note that intensity discrimination is finest when the test is made at the same intensity level as the adaptation. In general, this characteristic may be observed for all values of adapting intensity, and if the curves for a number of adapting intensities are plotted together on the same graph (e.g. Fig. 5 A, B or C), it can be seen that each curve tends to approach its minimum value of $\Delta I/I$ when test and adapt intensities are equal.

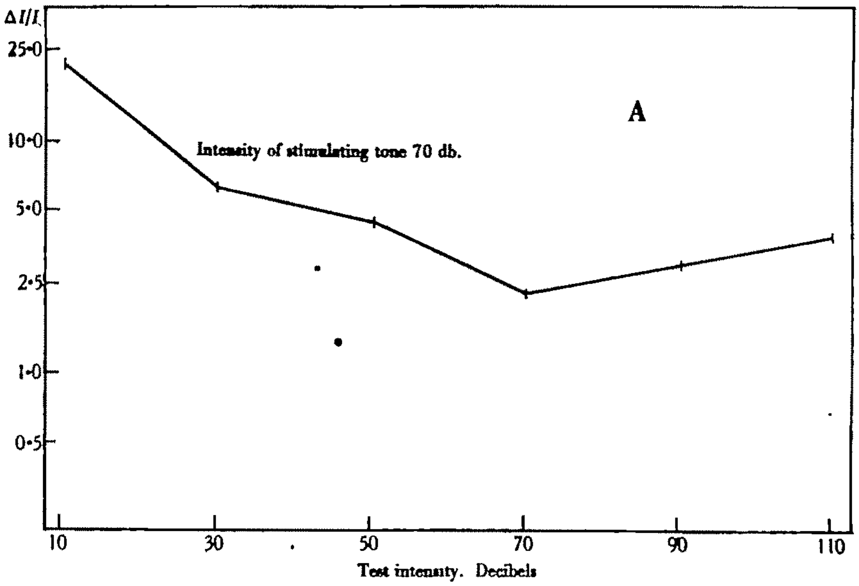

Fig. 4. Curve relating $\Delta I/I$ to test intensity, at 70 db. adaptation intensity. Frequency 1000 c.p.s.

The graphs obtained for each of the three observers are given in Fig. 5 A, B and C. It can be seen that the minimum value of $\Delta I/I$ at equal test and adapt intensities appears on nearly all the curves, but there are some exceptions, notably the 100 db.[1] adaptation curve for A (Fig. 5), which reaches a minimum at 70 db. test intensity. These exceptions would all disappear if one reading were altered: in the present

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[1] It must be understood that where this term occurs without qualification in connexion with intensity levels, it refers to decibels above the observer's own absolute threshold at the frequency employed: the decibel expresses a ratio, and not an absolute value.

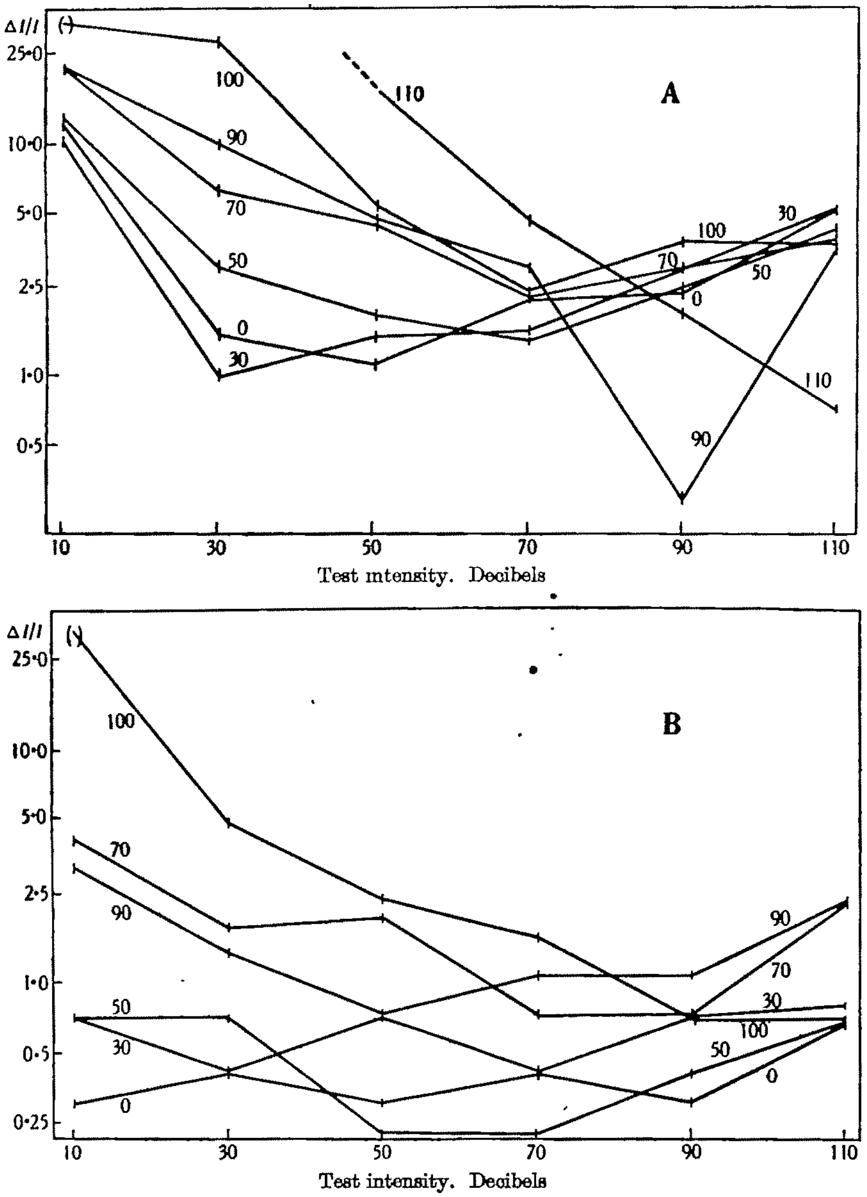

Fig. 5. Results of three observers A, B and C. The figures against each curve show the adaptation intensity in decibels above the absolute threshold. Frequency 1000 c.p.s.

instance, for example, if the 90 db. test had given a lower value of $\Delta I/I$ or the 70 db. test a higher value, the curve would have appeared typical. It is possible that such readings are in fact anomalous, due to wandering attention on the part of the observer, although he made no spontaneous acknowledgement at the time.

It may further be remarked that at low values of adaptation intensity (0, i.e. silence, and 30 db.), observers B and C (Fig. 5) show a very straight curve, while A gives a much higher value of $\Delta I/I$ at the lowest test intensities. It may be that observer A used a more certain criterion of the threshold than B or C at these low intensities.

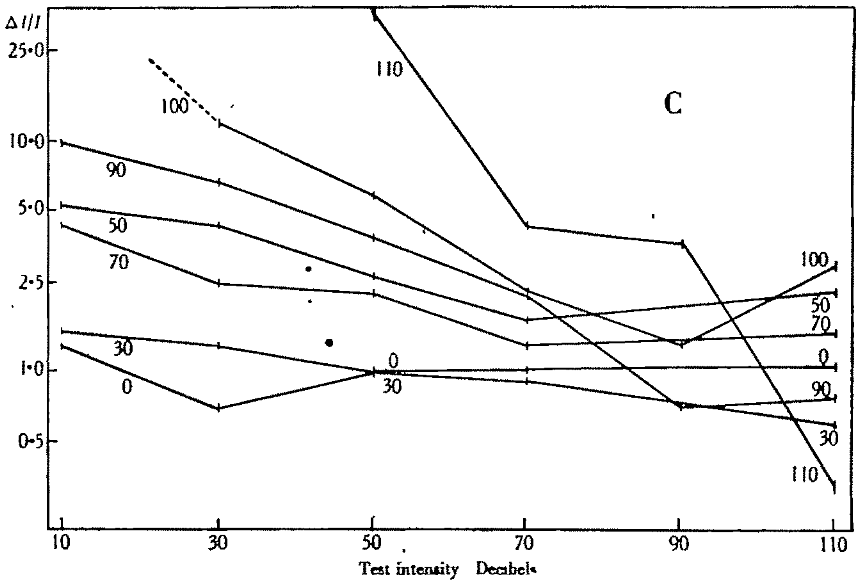

Fig. 5 C. See fig. 5 A, B for legend.

Such individual differences, however, are of minor importance by comparison with the general inference to be drawn from these curves; namely, that the region of the Weber's Law curve corresponding to the lowest values of $\Delta I/I$ shifts progressively towards those abscissae corresponding to high test intensities, when the ear is adapted to these high intensities. Indeed, the 110 db. adaptation curve reaches a very low value of $\Delta I/I$ at 110 db. test, and it may well be that it would rise again were it possible to test at higher intensities without causing serious pain to the observer.

It will be noted that with the higher intensities of adaptation, the differential threshold becomes very large at low test intensities, and was on occasion greater than the deepest modulation which could be obtained with the apparatus. This is almost certainly due to the fact that at these intensities of adaptation, the absolute threshold rises, often as much as 30 db. (Rawdon-Smith, 1934, 1936). Thus in fact the observer may not hear the minima of the modulation at all. This speculation is borne out, moreover, by subsequent experiment, in which just this effect was demonstrated. It follows that, at 100 and 110 db. adaptation, the differential threshold reading obtained at test intensities below 15–20 and 30–40 db. respectively is no more than a measure of the absolute threshold. In consequence, such readings have been bracketed in the graphs A, B and C (Fig. 5).

All the above experiments were done at a fixed frequency of 1000 c.p.s. A further series was undertaken, in which the ear was adapted to a 1000 c.p.s. tone at 100 db., and tested at an intensity 30 db. above the respective thresholds at 100, 250, 500, 1000, 2000 and 4000 c.p.s. The results of this series can be plotted in graph form (Fig. 6 A, B and C) and show the value of $\Delta I/I$ obtained at the various frequencies, first with adaptation to silence, and then with adaptation to a 1000 c.p.s. tone at 100 db. In each case, the 1000 c.p.s. adaptation tone had no marked effect at lower test frequencies; it showed the expected effect at the same frequency, a comparable effect at 2000 c.p.s., and a slightly smaller effect at 4000 c.p.s. It is evident, therefore, that the effect of previous adaptation on the differential threshold is not confined to the adaptation tone frequency. This was also shown (Rawdon-Smith, 1934) to be true of the absolute threshold.

V. CONCLUSIONS

In attempting to draw conclusions from the results which we have summarized above, it appears relevant to effect some comparison between these data and those derived from related investigations in the auditory and the visual senses. To certain earlier work on the effect of intense stimulation on the absolute sensitivity of the human ear we have already referred (p. 124), and we may here note simply that, like the effects there discussed, the loss of differential sensitivity described in the present paper presents, primarily, the characteristic of lability. Indeed, this and other characteristics of the phenomenon lead us tentatively to ascribe it, as in the earlier case, in larger part to centrally mediated than to more peripheral changes; though as in the former case a peripheral aspect may also be present. If this is true, it would appear that

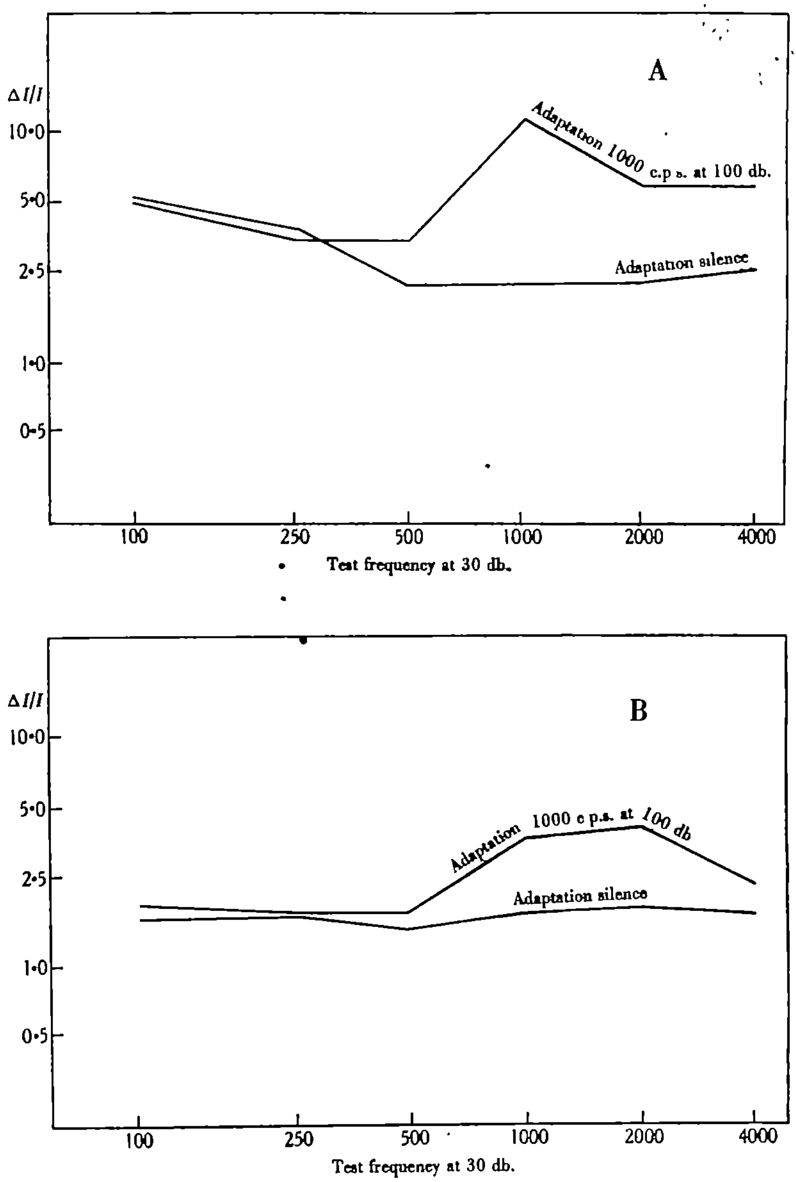

Fig. 6. Results of three observers A, B and C. Adaptation silence and 100 db. at 1000 c.p.s.; test 30 db. above threshold at the frequencies stated.

the differential threshold, like the absolute, is sensitive to what in an earlier paper (Rawdon-Smith, 1936) we have termed 'auditory inhibition'. Of this general phenomenon indeed, the loss of differential sensitivity described here may well be simply another aspect.

For the visual sense, Craik (1938) has recently shown an effect closely related to that which we have described; namely, a loss of differential sensitivity at low absolute intensities, after adaptation of the eye to a bright visual field. As in our case, differential sensitivity appears to be optimal when tested at an intensity close to that to which the sense organ has previously been subjected.

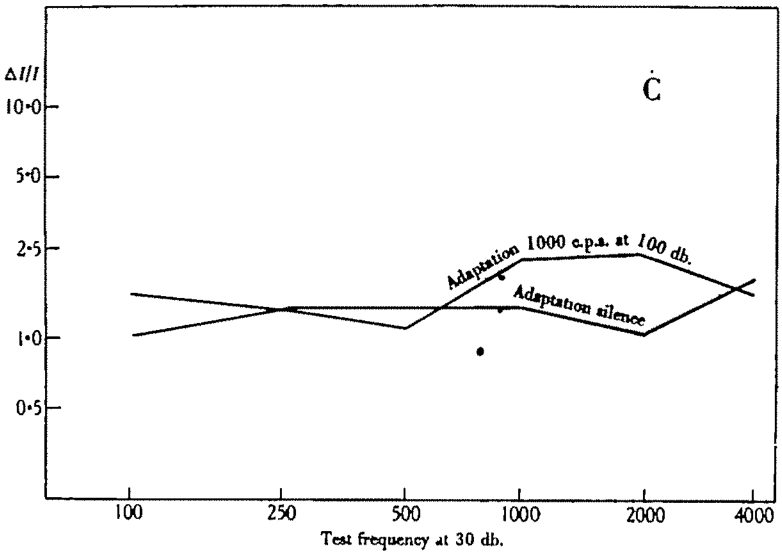

Fig. 6 C. See fig. 6 A, B for legend.

In many respects, indeed, the two senses—though in their mode of function so clearly different—here exhibit striking parallels. The significance of these for visual and auditory psycho-physiology, however, will be estimated with certainty only on the basis of further experimental work.

VI. SUMMARY

A new technique for the assessment of the least perceptible increase in sound intensity has been evolved, using a method whereby a slow rhythmic change in voltage can be generated, and this made to modulate the output from a simple pure tone source. It is possible thus to estimate the differential threshold over a wide range of sound intensities, and at any frequency which can be generated. It is further possible to measure the differential sensitivity at an intensity different from that to which the ear has been subjected for a previous period.

Using this technique, it has been found that the values of differential threshold differ greatly according to the state of adaptation of the ear: differential sensitivity is in general greatest when tested at the same intensity as that to which the ear has previously been adapted; it is less when the adapting intensity is greater or less than the test intensity.

It has further been found that adaptation at frequencies other than the test frequency does not have as great an effect as is observed when the adapting and test frequencies are the same, and often, indeed, no effect can be detected. Exceptions are found when the frequency of the adapting tone is equal to twice or four times that of the test tone.

Certain characteristics of these phenomena appear to indicate that the effect should be ascribed more readily to the intervention of central factors, than to modifications of sensitivity of the peripheral mechanism; this has also been shown to hold true of previously investigated effects on the ear's absolute sensitivity after stimulation. We believe, therefore, that like the latter, the effects discussed in this paper represent another aspect of the general phenomenon of auditory inhibition.

In part, the expenses of this investigation were defrayed by a grant from the Government Grant Committee of the Royal Society, to whom the authors wish to record their gratitude. They are also deeply indebted to Mr K. J. W. Craik, for assistance with the apparatus and as an observer.

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OBSERVATIONS RELATING TO THE THRESHOLD OF A SMALL FIGURE WITHIN THE CONTOUR OF A CLOSED-LINE FIGURE

By K. J. W. CRAIK and O. L. ZANGWILL

From the Cambridge Psychological Laboratory

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I. INTRODUCTION

KOFFKA, in his discussion of the problem of shape ((12), pp. 129–60), calls attention to the fact that if a line forms a closed or almost closed figure it will tend to be perceived not as a mere line upon a homogeneous background but as a surface figure bounded by a contour ((12), p. 150). A figure of this kind he therefore calls a *closed contour figure*. Koffka then goes on to predict that the threshold of a variable is likely to be higher when projected upon the surface of a closed contour figure than when projected outside the bounding contour. This prediction is based partly on general grounds and partly on the results of earlier experiments (7, 8) relating to differences of threshold on figure and ground.

Koffka points out that his prediction might readily be put to the experimental test. He suggests to this end a modification of a method originally devised by Hempstead (11). The proposed method consists essentially in projecting a small figure upon the contoured surface and arranging an episcotister between the latter and the projection lantern. The differential threshold of the small figure could then be determined by suitable adjustment of the variable episcotister aperture. "If then", writes Koffka, "the little figure required a greater episcotister opening in order to become visible inside than outside the contour, we should have proved a greater cohesiveness of the enclosed area as compared with its surroundings, which would make it more difficult to produce a new figure on it" ((12), p. 150). "Unfortunately", he concludes, "this experiment has never been made, although from two similar experiments, one by Gelb and Granit and the other by Granit, our assumed result seems predictable."

The purpose of the present study is to remedy the deficiency emphasized by Koffka and thereby to submit his theoretical prediction to the experimental test.

II. PRELIMINARY EXPERIMENTS

(1) *Apparatus and method*

A preliminary test utilizing the Hempstead technique (described above) was carried out which gave strong indications of a positive result. This technique is however unsatisfactory for a number of reasons: In the first place, a great deal of time is consumed in obtaining an adequate number of threshold determinations from each subject. In the second place, the noise engendered in varying the episcotister aperture during rotation has a somewhat distracting effect on the subject. In the third place, it is difficult to vary the aperture during rotation in a regular fashion. Consequently, we agreed to abandon the Hempstead technique and to determine the threshold of the variable figure with the aid of an iris diaphragm. The apparatus employed in these preliminary experiments was originally devised by Grindley (10) for group demonstration of Weber's Law, but it could readily be adapted for our present purpose.

The apparatus (Fig. 1) consisted of a lamp (*L*) placed at a distance of 1·8 m. from a white cardboard screen (*SS*) which it illuminates directly through the window (*W*). Some light from *L* is reflected on to the screen by the mirror (*M*) and this light passes through the lens (*O*). A small rectangle, 2 × 3 mm., was drawn in black ink upon an ordinary glass notice plate (*P*) interposed between *L* and *M*. An image of the rectangle is thrown on the screen, and the difference between its brightness and that of the background can be varied by means of the iris diaphragm (*I*) on the lens. The diaphragm was calibrated directly for $\delta I/I$ on a scale attached to the exterior of the instrument. The brightness of *L* was adjusted to a constant value for the purposes of the experiment. The value chosen for direct illumination of the screen was 0·50 foot-

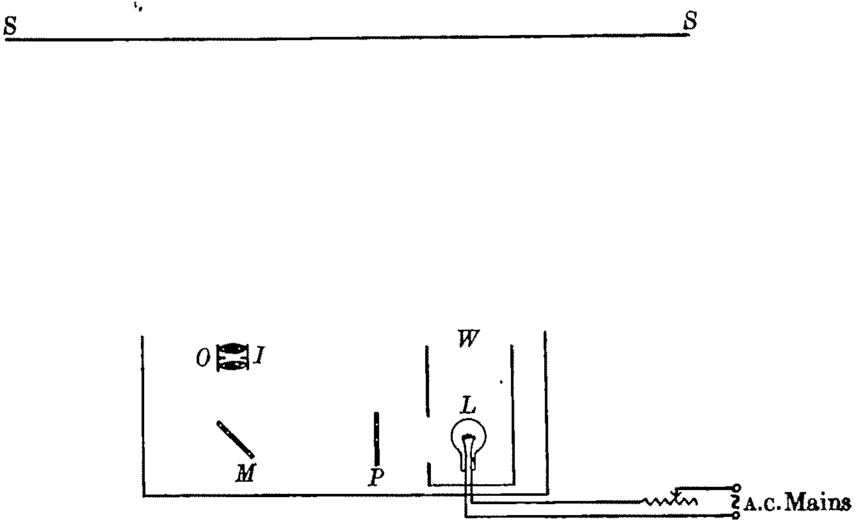

Fig. 1.

candles.[1] It is obvious that enlargement of the diaphragm aperture under these conditions will lead to a slight increase in total illumination of the screen. This is not, however, perceptible as a change in background illumination but as a difference between the brightness of the figure and the ground. The changes made by this method give the threshold as $\frac{\delta I}{I+\delta I}$ which is virtually equivalent to $\delta I/I$ for the small ratios of $\delta I : I$ employed.

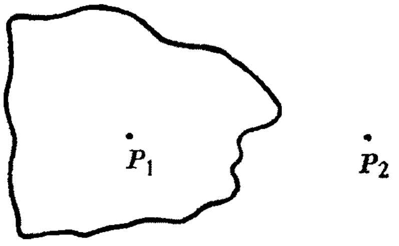

Fig. 2. Original size reduced by 0·5.

The screen SS was a white cardboard sheet, 25 × 20 in., suspended in such a way that it could be shifted horizontally in either direction. An irregular figure (Fig. 2) was drawn in black crayon at about the centre of the screen. For purposes of fixation, a small black point (P_1) was marked within the contour of the figure such that when the image of the rectangle is projected inside the border, this point approximately marks what would be the point of intersection of its diagonals. A second point (P_2), outside the border of the larger figure, marks the fixation point under conditions in which the image of the rectangle is arranged to fall outside the contour.

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[1] The illumination conditions were of course maintained constant for all positions of the variable figure. Subsequent determinations in which a higher value for the screen was adopted gave results identical with those reported below.

(2) *Procedure*

The subject was seated to the right of the apparatus in such a manner that his head and the lamp L were approximately level. The threshold was then determined in the following way: With the image of the rectangle projected on the contour figure, five readings of the threshold for 'disappearance' followed by five for 'appearance' of the variable figure were obtained. The screen was then shifted so as to bring the image of the rectangle on to a specified area of the screen outside the border of the contour figure. A further set of five readings for 'disappearance' and five for 'appearance' was then obtained in an exactly similar way. The screen was then shifted back to its original position and ten further readings obtained as before. Finally, a set of ten determinations was secured with the image of the rectangle falling once again in its previous position outside the contour.

Special precautions were exercised with reference to (*a*) the notorious lack of definition of the differential threshold; (*b*) the rate of appearance and disappearance of the variable; and (*c*) the possible effects of practice and fatigue. Thus in the first place, we instructed each subject to adopt and maintain as distinctive a criterion of the threshold as he was able. In the second place, we endeavoured to operate the diaphragm at a constant rate during any one given determination, but to vary the rate slightly as between the successive determinations. We also instructed the subjects to go as little as possible by time interval in effecting their judgements as this would not remain constant for all trials. In the third place, we gave rest pauses wherever necessary in order to reduce to a minimum interference by after-imagery. And in the fourth place, as already described, we distributed the trials under the two sets of conditions to offset any differential effect of practice upon discrimination.

(3) *Subjects*

Twenty subjects, the majority of whom were either research workers or students in psychology, took part in the experiment. With two exceptions, all were unaware of the purpose of the investigation.

(4) *Results and discussion*

The mean value of the differential threshold of the small figure when projected within the contour of the larger figure was found to be higher than the mean threshold of the same figure when projected outside the contour in the case of nineteen out of the twenty subjects. The mean value of the difference between the two sets of readings for all subjects was found to be statistically significant. This result provides a clear confirmation of Koffka's predicted finding. It does not, however, necessarily establish the validity of Koffka's theoretical position. Our result might equally well be due to the operation of some differential contrast effect. Further experiment is therefore required to establish the origin of the phenomenon under discussion.

III. FINAL EXPERIMENTS

(1) *Object and method*

A final, and we hope decisive, series of experiments was planned to determine whether our preliminary result was due to the intrinsic cohesion of a contour figure or whether it derives from some process allied to contrast.

Our method is described below:

Two series of drawings were prepared. Those forming the *Figural* (*F*-) *Series* are shown in Fig. 3; those forming the *Control* (*C*-) *Series* in Fig. 4. The procedure involved a series of threshold determinations of a small patch of variable intensity at the centre of the square shown in Fig. 3*d* or at a strictly corresponding position in the case of each of the remaining figures shown below.

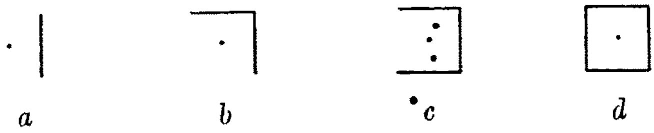

Fig. 3.

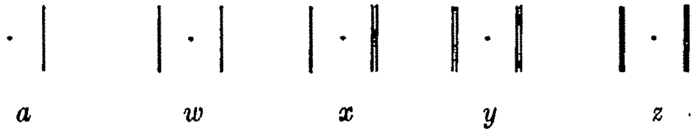

Fig. 4.

It is to be expected on Koffka's view that the threshold of the variable would show *either* a sudden rise on complete closure of the square (*F*-series, stage *d*) *or* a progressive rise with increasing figural closure (*F*-series, stages *c*, *d*). If the first alternative obtains, one would expect no corresponding terminal rise of threshold in the case of the *C*-series; if the second obtains, one would predict consistently higher threshold values for stages *c* and *d* in the *F*-series as compared with stages *x*, *y*, and *z* in the *C*-series.

If, on the other hand, the entire effect is due to contrast, no such differences as the above would be expected, since the size and distance of the lines in corresponding members of the two series are identical.

In conclusion, to check the analogy between the present conditions and those obtaining in the preliminary experiment, a limited set of values of the threshold inside and outside the contour of the square was secured.

(2) *Apparatus*

The apparatus (Fig. 5) consisted of a set of nine identical white card screens (S_1), each 20 × 13 in., made up in book form, hinged at one side and fitted with a pin to register them accurately at the other side. In the centre of each screen was a very small fixation point. The first screen was otherwise blank; the others had one or more Indian ink lines each 1 in. long and ½ in. from the fixation point, as shown in Figs. 3 and 4 above. The last member of the *C*-series consisted of two lines having twice the width of those in the other fields and consequently equivalent in black area to a four-line figure.

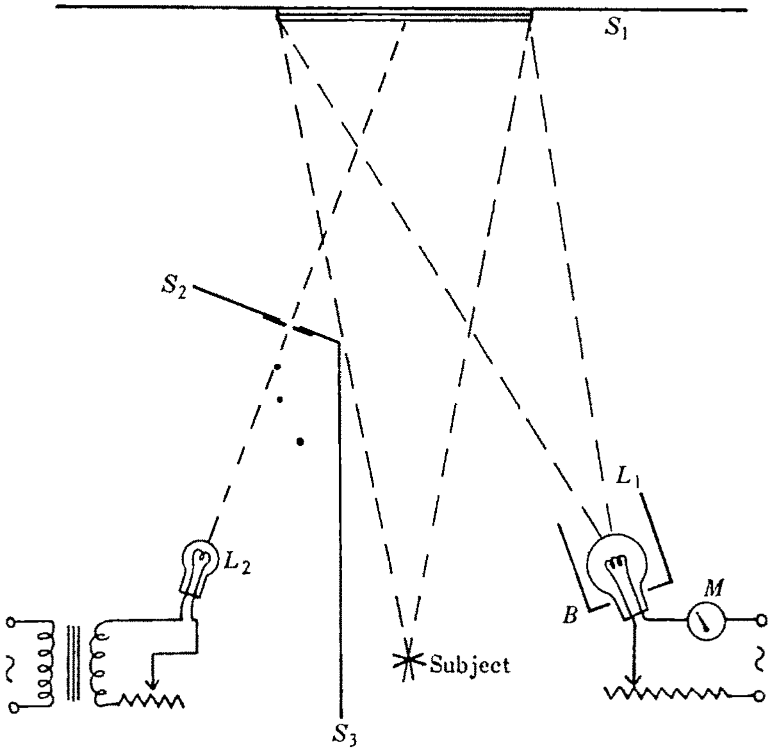

Fig. 5.

The screen was illuminated by a 12 V. 24 W. bulb (L_1), supplied from the A.C. mains through a resistance. Its illumination was sufficiently uniform over the field and was arranged to be 1·5 foot-candles. The current through the lamp was measured continuously by an ammeter (M) and found constant to 2%. The lamp was shielded from the subject by a box (B). On the left of the subject, who sat in a fixed chair with his eyes at a distance of 50 in. from the screen, was a 4 V. bulb (L_2) with a compact filament casting on the screen S_1 a sharp bright shadow of a small rectangular aperture in the screen S_2. The position of the bright shadow could be adjusted to coincide exactly on each screen by having the aperture in a movable metal slide on S_2. This bulb was supplied by a transformer from the mains and its brightness was varied by means of a 25 ohm series resistance having a very smooth and silent motion; this gave a range of illumination on the screen from 0·002 to 0·15 foot-candles. The resistance and the bulb were screened from the subject by S_3. The brightness of the variable illumination on the screen was measured by a Holophane Lumeter and checked by an Everett Edgcumbe Luxometer, and the resistance calibrated directly in foot-candles. Although varying the brightness by a series resistance alters the colour of the light considerably, its total brightness is never more than 10% and its threshold brightness 2% or less, so that the change of colour is lost in the main illumination.

The experiments were carried out in a dark room with a very small lamp continuously illuminating the resistance scale and the experimenter's record of the readings.

The rectangular screen subtended angles of 20° × 15° at the subject's eye; the complete figure, 1° 12′ sq. and the test patch, 24′ × 10′.

(3) *Procedure*

Eight subjects were employed in the main experiment. These comprised five research workers and three students in psychology.

In the case of five of the subjects, the determinations were made in the following order: Blank field; *F*-series in the sequence *a*, *b*, *c*, *d*; blank field; *C*-series in the sequence *w*, *x*, *y*, *z*; blank field. In the case of the remaining three subjects the whole order of presentation was inverted.

Ten threshold values were determined for every figure, five 'appearing' and five 'disappearing'. A total of ten readings was likewise taken in the case of every determination on the blank field.

The subject was instructed to say 'stop' when the brightness of the variable reached threshold value. In order to obviate judgements based on time interval, the experimenter in changing the brightness endeavoured to vary both the speed and the starting-point of each determination.

(4) *Results*

The mean threshold value of each subject in the case of the blank field and of each of the two series of test fields is given in Table I. The symbols (*s*) and (*i*) stand for 'straight' and 'inverted' order respectively.

The ratio of the threshold on each field to the mean threshold taken as unity is shown in Table II.

It will be seen from Table II that the mean differential threshold values at equivalent stages of the two series are identical. This result is also given graphically by Fig. 6.

The mean variation, indicative of degree of scatter, is 0·17.

Table I

Subject	Field							
	Blank	*a*	*w*	*b*	*x*	*c*	*y, z*	*d*
R.H.T. (*s*)	0·044	0·061	0·049	0·066	0·065	0·072	0·075	0·079
R.C.O. (*s*)	0·019	0·032	0·028	0·041	0·036	0·037	0·035	0·035
M.H.H. (*i*)	0·041	0·041	0·067	0·060	0·061	0·058	0·062	0·065
M.G.C. (*s*)	0·081	0·078	0·105	0·094	0·130	0·100	0·122	0·104
A.T.W. (*s*)	0·066	0·064	0·088	0·079	0·095	0·093	0·100	0·093
O.L.Z. (*s*)	0·023	0·029	0·041	0·027	0·039	0·035	0·040	0·034
K.J.W.C. (*i*)	0·029	0·035	0·036	0·044	0·042	0·047	0·046	0·052
J.G.F. (*i*)	0·081	0·086	0·092	0·096	0·100	0·101	0·108	0·108

Table II

Subject	Field						
	a	*w*	*b*	*x*	*c*	*y, z*	*d*
R.H.T.	1·4	1·1	1·5	1·5	1·6	1·7	1·8
R.C.O.	1·7	1·5	2·1	1·9	1·9	1·8	1·8
M.H.H.	1·0	1·6	1·5	1·5	1·4	1·5	1·6
M.G.C.	0·9	1·2	1·2	1·6	1·2	1·4	1·3
A.T.W.	1·0	1·3	1·2	1·4	1·4	1·5	1·4
O.L.Z.	1·3	1·8	1·2	1·7	1·5	1·7	1·5
K.J.W.C.	1·2	1·2	1·5	1·5	1·6	1·6	1·8
J.G.F.	1·1	1·1	1·2	1·2	1·2	1·3	1·3
Mean	1·2	1·4	1·4	1·5	1·5	·6	1·6

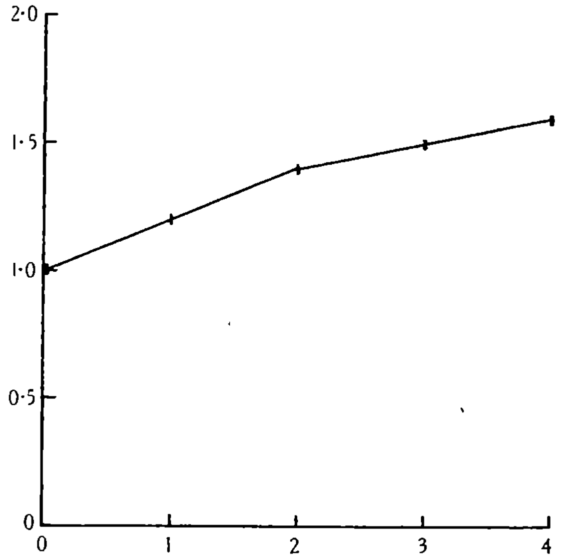

Fig. 6. Ordinate: mean ratio of threshold near lines to threshold on blank field where latter is taken as unity. Abscissa: number of lines in the field.

An additional set of threshold determinations inside and outside the contour of the square is presented in Table III. Each value in the first column of this table constitutes a mean threshold value of the variable in the position shown in Fig. 3 *d*. Each value in the second column constitutes the mean for a set of threshold determinations with the variable ½ in. to the left of one vertical side of the square and a set with the variable ½ in. to the right of the other vertical side. The ratio 'inside'/'outside' is shown in the third column.

Table III

Subject	Inside (i)	Outside (o)	i/o
K.J.W.C.	0·041	0·028	1·5
O.L.Z.	0·041	0·029	1·4
M.D.V.	0·051	0·036	1·4

(5) *Discussion*

The results of our final series of experiments clearly show that neither expectation set up by Koffka's cohesion hypothesis is satisfied. In the first place, there is a *flattening* at the final stages of the curve for the *F*-series. In the second place, the curves for the two series actually coincide throughout. We thus seem bound to conclude that the rise in threshold noted under our experimental conditions cannot be due to the intrinsic cohesiveness of an enclosed area.

Our conclusion finds a measure of support in the results of some recent experimental studies. Thus Fry & Robertson, in their re-examination of the alleged influence of figural properties upon hue and brightness, are led to conclude that "...the observed effects instead of being due to the mode of perception are due rather to neglected factors in the conditions of ocular stimulation" ((6), p. 424). Special attention was devoted by these authors to the problem of the differential brightness threshold on figure and ground. "We have avoided the difficulties inherent in Gelb and Granit's method", they claim, "by using a pattern ...whose parts can be seen interchangeably as figure and ground, without involving any change in the stimulus conditions, and in such a case we have found that there is no significant difference between the threshold on figure and on ground" ((6), p. 434). This result, when taken in conjunction with our own findings, would seem to indicate that the indubitable rise in threshold observed in our own experiments, as also in certain of those of Fry & Robertson ((6), pp. 426–7) and Fry & Bartley ((5), pp. 415–16), must be attributed not to "differences in perceptual relationships" but to "changes produced in the peripheral physiological processes" ((6), p. 426).

We must concede that very little is known at present as to the precise nature of these peripheral changes. But there is already ample evidence of very marked effects on the differential brightness threshold due to a dark surrounding field (Abribat (1), Cobb (2), Lythgoe (13), Lythgoe & Tansley (14)). These findings, together with those of Craik (3, 4), indicate that a somewhat dark adapted retina is ill-equipped to discriminate differences at a fairly high illumination. It is tempting, and not altogether speculative, to suppose that a dark figure (or 'surround') causes some degree of dark adaptation in neighbouring areas of the retina akin to that produced by subjection of the *whole* retina to a period of comparative darkness. For as one of us (Craik) has pointed out elsewhere, "...spatial and temporal proximity of an adapting field appear in general to have very similar effects" ((4), p. 252). We may note also that Schouten (16) and Schouten & Ornstein (17) come to a similar conclusion.[1]

The general implication of all this work would seem to us to be that the threshold effects discussed above cannot profitably be treated as functions of 'organization' in Koffka's sense of the term. In the first place, as we have endeavoured to indicate, other and more appropriate lines of explanation are available. In the second place, there seem to us intrinsic reasons why the threshold technique is fruitless when applied to the investigation of figural properties. It is our belief that the processes underlying differential brightness sensitivity are peripheral in origin. They must in consequence be treated by the organism at the lowest levels of functional integration. Form perception, and the apprehension of figural properties, we believe to be founded on activities located at higher integrative levels (cf. Parsons ((15), pp. 45–60)). Nevertheless, there seems to us no good reason why the high level processes which find their expression in the emergence of form need necessarily obscure those whose determinants are purely peripheral. Thus in cases such as that reported in the present study, it would seem to us idle to attempt to establish the phenomenal reality of an attribute of shape by means of the threshold technique. Prof. Koffka's theory of perception, despite its brilliant synthetic qualities, seems to us severely limited by its author's persistent refusal to countenance any principle of 'levels' in the perceptual process.

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[1] Granit (9) has recently been able to show that the differential brightness sensitivity of the retina, as measured by its electrical response, is decreased (at moderate illuminations) by previous dark adaptation.

IV. SUMMARY AND CONCLUSIONS

1. The threshold of a small rectangle of variable brightness is significantly higher when projected inside the contour of a larger irregular closed figure than when projected outside the contour. A similar result is obtained in the case of a small patch of variable intensity projected inside and outside the contour of a square.

2. These results substantiate Koffka's prediction that the differential threshold of a variable will be raised by its projection on a contoured surface. They do not, however, provide unequivocal support for his theory of the intrinsic cohesiveness of a closed contour figure.

3. The outcome of a crucial experiment shows that there is a progressive rise in threshold of a variable as black lines of specified dimensions are added to the field in which the brightness change occurs. It is however pointed out that the rise is *identical* whether the lines are arranged to build a closed square or merely to constitute two sets of parallel lines.

4. It is concluded from these results that the observed rise in threshold cannot depend upon Koffka's factor of intrinsic cohesion. An alternative interpretation, more in harmony with the known facts of visual adaptation, is tentatively advanced. Certain broader implications of the results are briefly discussed.

The writers are profoundly indebted to Prof. K. Koffka for the interest which he has taken in these studies. They alone, however, are responsible for the theoretical interpretations presented above, to which Prof. Koffka does not himself subscribe. Gratitude is also due to Prof. F. C. Bartlett for his encouragement and help throughout.

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SOME FACTORS OF PERSONALITY.

A FURTHER ANALYSIS OF SOME OF WEBB'S DATA

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From the Psychological Laboratory of the University of Capetown

I. *Introduction* (pp. 151–154).
II. *Further analysis of Webb's data* (pp. 154–165).
  (1) *Traits selected* (pp. 154–155).
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  (4) *Conclusions as to qualities isolated* (pp. 162–165).
References (p. 165).

I. Introduction

One of the main problems in the analysis of temperament and character is that of finding a suitable framework in which to set the results of observation and experiment. A great deal of activity has been shown by a variety of authors in attacking this problem, but unfortunately no general agreement has been reached. A factor proposed by one author may be accepted by another, with or without modification. Thus, for example, the factor '*w*' which Webb isolated in his classic study on "Character and Intelligence" (11), has been recognized, with some alterations, by Garnett (5) and by Cattell (3). The factor '*c*', isolated also from Webb's material, was named 'cleverness' by Garnett, and stands obviously in close relation both to the Surgence-Desurgence factor of Cattell and to the Dominance-Submission factor of the Bernreuter Personality Inventory (1), although Spearman (8) would prefer to regard it as the inverse of perseveration. But on the other hand, although single factors are common to several schemes, they are supplemented in different ways by different authors, and there is no common framework which everyone can accept.

This difficulty arises in part from a weakness that characterizes more than one of the methods of analysis that have been used. Procedures based on the technique devised by Spearman involve the selection of a relatively small group of traits such that their intercorrelations will satisfy the tetrad equation criterion. A single factor is then postulated, and an attempt made to identify it. Then, the influence of this factor being partialled out, a new grouping is made, and a further factor is extracted. A result of this method is that when a factor is discovered its nature is assumed to be fixed, and is not reconsidered in the light of the factors that are isolated afterwards. That is to say, a number of reference axes are laid down, one after another, each being fixed by itself, no attempt being made to discover the best position for the co-ordinate frame as a whole. And yet it is obvious that the initial positions of these axes are only provisional, and may have to be altered in the light of further data.

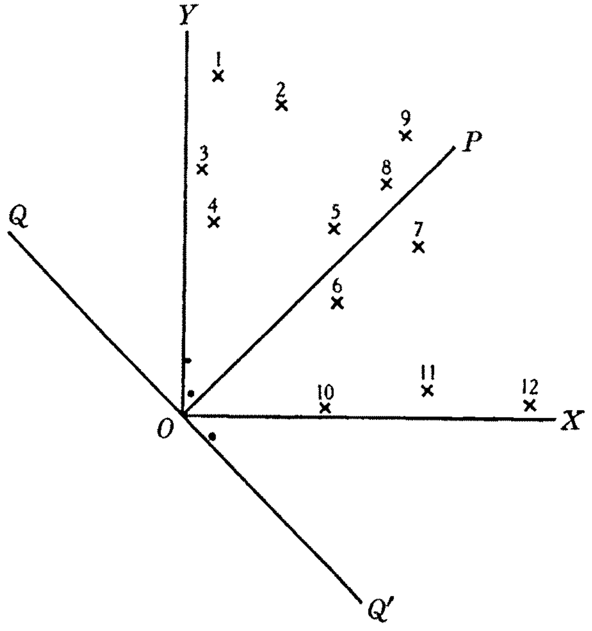

Fig. 1.

For example, suppose there are twelve traits having two independent common factors such that the factor loadings are either positive or zero, and let these two factors be represented by the co-ordinate axes *OX* and *OY* in Fig. 1. Suppose further that the technique we have been discussing is applied to this material, and traits 5, 6, 7, 8 and 9 are considered as a group for preliminary analysis. Within the limits of the probable error, the correlations between these traits satisfy the tetrad criterion, and consequently a single common factor is announced. It is indicated in the diagram by the axis *OP*, drawn from the origin through the points to fit them most closely. But when the remaining traits are considered, it is clear that this axis does not represent a single primary quality. When a second axis is fitted to explain the remaining intercorrelations of the whole group of traits, it must be drawn at right angles to *OP* in order to preserve the independence of the factors. Let this axis be represented by the line *OQ*. It follows at once that the loadings of about half the traits are negative, and that is in contradiction with our postulate that all factor loadings are either positive or zero. The difficulty is entirely removed if the axes are rotated from *OP*, *OQ* to the positions *OX*, *OY*. And when this is done, the supposed factor *OP* disappears.

This example is given to illustrate a general principle. The particular assumption made for convenience of exposition, namely, that there are no negative factor loadings, is not intended to apply beyond this imaginary case, and is merely used to represent *any* relevant psychological considerations. The underlying principle is that the final positions of all the axes of reference can be determined only when they are all considered together. If the significance provisionally given to the factors first discovered renders it difficult, or even impossible, to give a reasonable psychological interpretation to others that are later found necessary to complete the explanation of the material, then the axes as a whole must be rotated in such a way that a meaning can be given to them all.

Even when procedures more powerful than those derived from the tetrad equation criterion are employed, this consideration is not always given due weight. The point is well expressed, not for the first time, by Thurstone (10).

> One of the most common characteristics of factor studies is the computation of a factorial matrix and the attempt to make a psychological interpretation without rotating the reference frame. It does not matter whether the factorial matrix has been produced by the centroid method or by the principal axes or components. In either case the configuration must be rotated before the factors can be even expected to have psychological meaning. One cannot expect to find several meaningful factors by any of the current methods of factoring a correlational matrix without rotation of the reference frame. (p. 90.)

Perhaps an illustration will make this clearer. Taking from Brigham a table of the intercorrelations of marks obtained by officers and cadets of the U.S. Military Academy in eleven subjects, viz. English, History, Economics, Law, Engineering, Ordnance, Physics, Chemistry, Mathematics, Surveying and Drawing, Flanagan (4) analyses it by Hotelling's method. Only the first two factors need concern us here. Taking them as they stand, the author is driven to describe them thus:

> We might call the first factor a general-achievement factor. None of our more elemental concepts seems to fit it particularly well. Evidence from other studies of achievement would lead one to believe that a major part of this may be considered to

> be due to industry, which is itself a complex of such things as interest, purpose, temperament, perseverance, motivation, etc. Probably Spearman's '*g*' may also be thought of as contributing quite substantially to this first factor.
>
> The second factor...has its largest factor-loading in drawing, with surveying and mathematics next in order. The largest negative factor-loading is for history, followed by English, economics and law. This second factor seems, then, to represent a difference between spatial ability, perhaps combined with some numerical ability and a certain amount of manual dexterity, and verbal ability, with possibly some memory involved. As a further speculation we suggest that it might be linked with some physiological or temperament factor which gave mental activities a singleness of direction and resistance to change very similar to the inertia of a moving body. (p. 33.)

Could the case for rotation be stated better? If the loadings of these two factors are plotted, and the axes then turned through a negative angle of 29° or 30°, the picture changes entirely. We now get one factor with negligible loads on history, economics, English and law, and with significant loads on engineering, ordnance, physics, chemistry, mathematics, surveying and drawing—obviously not an impossible situation to interpret; and a second factor with a negligible load on drawing, with heavy loadings on English, history, economics and law, and with intermediate loadings on the other subjects—again a factor of reasonably plain meaning. Further rotation in a larger number of dimensions might improve the situation still more, but enough has been said to show the undesirability of trying to give a psychological interpretation to the products of factorial analysis as they come straight from the mathematical mill.

II. FURTHER ANALYSIS OF WEBB'S DATA

(1) *Traits selected*

In view of these considerations it struck us that before making use of material gathered by ourselves it might be advisable to re-examine the data collected by Webb. Webb's material, however, is very extensive, and the factorial analysis of the whole of his correlation matrix seemed to us to be too large a task until we had more guiding posts than are at present available. Accordingly we selected nineteen of Webb's traits for further treatment. In his analysis '*g*' plays a fairly large part, but we had no desire to examine this factor in the present enquiry. We therefore began by selecting the eight traits by means of which Webb attempted to establish the existence of '*w*', since from his figures it is clear that '*g*' plays a very small part in producing their intercorrelations. To these we added eleven other traits which seemed to have a bearing on emotional disposition and behaviour. And as the results obtained by Webb for his groups of men students are more extensive and probably more reliable than those for his groups of schoolboys, we have employed the former throughout.

The following list gives the names of the traits in question, and the numbers attached to them by Webb in his schedule:

1. General tendency to be cheerful (as opposed to being depressed and low-spirited).
2. Tendency to *quick* oscillation between cheerfulness and depression (as opposed to permanence of mood).
3. Occasional liability to extreme depression.
4. Readiness to become angry.
5. Readiness to recover from anger.
6. Occasional liability to extreme anger.
8. Degree of sense of humour.
11. Eagerness for admiration.
15. Fondness for large social gatherings.
17. Impulsive kindness (to be distinguished from no. 18).
18. Tendency to do kindnesses on principle.
19. Degree of corporate spirit (in whatever body interest is taken, e.g. college, school, country, native place, etc.).
20. Trustworthiness (keeping his word or engagement, performing his believed duty).
21. Conscientiousness (keenness of interest in the goodness and wickedness of actions).
23. Readiness to accept the sentiments of his associates.
27. Degree of 'tact' in getting on with people.
31. Degree of bodily activity in pursuit of pleasure (games, etc.).
33. Tendency *not* to abandon tasks in the face of obstacles.
34. Tendency *not* to abandon tasks from mere changeability.

(2) *Method employed*

Turning now from the material, we come to the problem of method. Of the various devices available we have adopted that of Thurstone (9). Thurstone himself has stated clearly the limitations and weaknesses of his centroid method of factorizing a correlation matrix, but it is the simplest procedure, and we are not satisfied that the advantages claimed for such other methods as those of Kelley, Hotelling or Burt (2) are sufficient to compensate for their greater complexity. The positions of the axes as reached by the different methods are not identical, but if the different frames of reference are each orthogonal, it is possible to pass from one to another by suitable rotations. We have already seen that the axes have to be rotated in order to give them psychological significance. There is therefore no special virtue in any particular starting point. The difference

Table I. *Correlation matrix extracted from Webb's Table IV.*

Decimal points are omitted.

	1	2	3	4	5	6	8	11	15	17	18	19	20	21	23	27	31	33	34
1		−27	−53	−10	43	−16	68	12	51	20	19	42	07	−08	−01	21	36	23	06
2	−27		48	42	−27	38	01	31	10	−03	−43	−06	−43	−29	25	−12	05	−52	−48
3	−53	48		24	−33	21	−35	04	−30	−18	−26	−22	−14	−09	−06	−04	−27	−49	−24
4	−10	42	24		−63	70	11	65	49	−21	−81	−07	−55	−58	−45	−38	24	−21	−31
5	43	−27	−33	−63		−41	22	−32	02	41	48	32	45	33	26	48	−02	15	31
6	−16	38	21	70	−41		−13	53	26	−05	−34	03	−38	−39	−31	−32	23	−18	−30
8	68	01	−35	11	22	−13		12	55	34	26	46	01	−07	−19	42	30	23	−09
11	12	31	04	65	−32	53	12		36	03	−41	−12	−51	−51	−34	−26	25	−27	−31
15	51	10	−30	49	02	26	55	36		29	−04	51	−37	−49	−35	04	55	−13	−31
17	20	−03	−18	−21	41	−05	34	03	29		50	34	30	26	21	34	10	01	06
18	19	−43	−26	−81	48	−34	26	−41	−04	50		50	76	71	27	48	04	35	36
19	42	−06	−22	−07	32	03	46	−12	51	34	50		28	22	04	36	54	21	09
20	07	−43	−14	−55	45	−38	01	−51	−37	30	76	28		80	16	33	−13	39	53
21	−08	−29	−09	−58	33	−39	−07	−51	−49	26	71	22	80		24	27	−22	39	52
23	−01	25	−06	−45	26	−31	−19	−34	−35	21	27	04	16	24		12	−09	−17	00
27	21	−12	−04	−38	48	−32	42	−26	04	34	48	36	33	27	12		−02	10	13
31	36	05	−27	24	−02	23	30	25	55	10	04	54	−13	−22	−09	−02		−01	−12
33	23	−52	−49	−21	15	−18	23	−27	−13	01	35	21	39	39	−17	10	−01		63
34	06	−48	−24	−31	31	−30	−09	−31	−31	06	36	09	53	52	00	13	−12	63	

between the various systems is, however, too large a problem to discuss here, and merits separate consideration.

The correlation matrix which we have analysed is given in Table I.

One of the main problems in the application of any method of factorial analysis to fallible data is that of deciding how many factors should be extracted. If the material is free from error, the rank of the matrix can be accurately determined, and hence the number of factors to be extracted is known. But when the correlations are subject to error, the residuals may not vanish completely until as many factors have been taken out as there are variables in the correlation table. In other words, the common factor space cannot be separated from the error space, and the analysing mechanism will continue to operate until both have been exhausted.

It seems clear that the process should continue so long as the residuals are too large to be accounted for by chance, and that, on the other hand, in accordance with the principle of parsimony, it should stop when they have been so far reduced that the probable errors of the correlation coefficients are sufficient to account for them. In order to decide when this point had been reached, we thought it desirable to divide the residuals at each stage by the probable errors of the original coefficients, and to plot a frequency distribution of the results, disregarding signs. Treating this curve as one-half of a total distribution, we compared it with a normal curve covering the same area and having a standard deviation of 1·4825. If the fit is poor, it follows that something more than the operation of pure chance is required to account for the residuals.

(3) *Results obtained*

After three factors had been extracted, the residuals by this test proved to be too large, and after the fourth factor they were too small. The values of P yielded by Pearson's test for goodness of fit were, for the third factor residuals 0·17, and for the fourth factor residuals 0·07. As it is not possible to extract three and a half factors, we decided to retain the fourth, and to recognize that a certain amount of error space is mixed up with the common factor space.

The matrix M, derived by the centroid method from Table I, is presented in Table II. The first four columns give the factor loadings of the nineteen traits analysed, and the fifth column, h^2, gives the communalities of the traits.

As it stands this matrix has no psychological significance, and its axes require to be rotated until they represent recognizable psychological factors. As a starting point we took the factor 'c' already isolated from

Table II. *Centroid matrix* M

	I	II	III	IV	h^2
1	0·344	0·665	−0·117	−0·320	0·667
2	−0·520	0·015	0·436	0·207	0·504
3	−0·415	−0·374	0·337	0·221	0·475
4	−0·838	0·309	−0·308	0·168	0·921
5	0·672	0·155	0·196	−0·105	0·525
6	−0·618	0·261	−0·178	0·375	0·622
8	0·231	0·755	0·063	−0·210	0·671
11	−0·584	0·403	−0·171	0·025	0·533
15	−0·188	0·875	0·061	0·014	0·805
17	0·373	0·373	0·261	0·210	0·390
18	0·837	0·077	0·133	0·287	0·807
19	0·336	0·608	0·074	0·414	0·659
20	0·788	−0·186	−0·121	0·340	0·786
21	0·739	−0·311	−0·074	0·378	0·791
23	0·289	−0·200	0·409	0·083	0·298
27	0·510	0·232	0·375	−0·041	0·455
31	−0·065	0·555	−0·067	0·193	0·354
33	0·534	0·062	−0·600	−0·036	0·650
34	0·562	−0·180	−0·465	0·044	0·566

Webb's material by Garnett (5) and more fully described by Cattell (3). It is clear from the work of both these authors that this factor is strongly represented in traits 1 and 8, *cheerfulness* and *sense of humour*. As a preliminary measure we decided to pass one axis through the point representing trait 1, and at the same time to rotate the remaining axes in such a way as to preserve the orthogonality of the matrix. The first part of this task offers no difficulty; it is achieved by normalizing the co-ordinates of the point through which the axis is to be passed. This gives the direction cosines of the new axis referred to the axes of the centroid matrix. The first column of the new matrix is obtained by post-multiplication of M by

$$\left|\begin{array}{r} 0{\cdot}418 \\ 0{\cdot}808 \\ -0{\cdot}143 \\ -0{\cdot}389 \end{array}\right|$$

The second part of the task is a little harder. The problem can be stated in geometric terms as follows. Given the r direction cosines of one axis in an r-dimensional space, it is required to find any orthogonal transformation which preserves the direction cosines of the given axis.

A general solution of this problem has been given by E. Schmidt (7), but as it involves the evaluation of r^2 determinants of the order $(r-1)$, we looked for a simpler method. Fortunately, *any* transformation will suffice which preserves the given axis and satisfies the criteria of orthogonality. In the four-dimensional case, with which we are immediately concerned, a solution can always be found by writing the given direction cosines a,

b, c, d, in four different orders, and adjusting the signs to satisfy the conditions of orthogonality, as in Table III. It will be seen that the cross product of any two columns in Table III is equal to zero, and if the sum of the squares of the elements in the first column is equal to unity, so also is the sum of the squares of every other column. This, of course, is not the only possible arrangement, but since we are concerned for the moment only with the first column, it is immaterial which of the many possible orthogonal transformations is employed.

Table III

$$\begin{vmatrix} a & b & -c & -d \\ b & -a & d & -c \\ c & d & a & b \\ d & -c & -b & a \end{vmatrix}$$

The method we suggest here is of particular value when one wishes to make use of any of the current methods of locating a single axis, such as Thurstone's single hyperplane method, and at the same time to transform orthogonally. It may be of use therefore to show how it can be extended to any number of dimensions.

The solution for r dimensions depends on the fact that the product of two or more orthogonal transformations is itself an orthogonal transformation. If each of these involves the rotation of four axes by a transformation of the type shown in Table III, and if each of the r axes is rotated by at least one of these transformations, then each of the terms appearing in the several transformations is represented in the first column of the product. By equating the elements in this column to the given direction cosines, we arrive at a series of linear equations whose solution yields numerical values for all the terms used, and substitution of those values in the product gives the required orthogonal transformation. We present a solution of the seven-dimensional problem by way of illustration. Table IV shows that a seven-dimensional orthogonal transformation can be written in terms of eight values, and that each of these values is represented in the first column of the transformation product T. If the given direction cosines are a, b, c, d, e, f, g, put $x=a$, $y=b$, $z=c$, $pw=d$, $qw=e$, $rw=f$, $sw=g$. Then $w=\sqrt{\{(1-(a^2+b^2+c^2)\}}$, $p=d/w$, $q=e/w$, $r=f/w$, $s=g/w$. Substitution of the resulting values of x, y, z, w, p, q, r and s in T gives an orthogonal transformation in which the first column contains the given direction cosines.

The number of transformations that have to be multiplied together is $(r-1)/3$, or the nearest integral number above $(r-1)/3$, where r is the

Table IV

$$\underset{T_1}{\left|\begin{array}{ccccccc} 1 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 & 0 & 0 & 0 \\ 0 & 0 & 1 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & p & -q & -r & -s \\ 0 & 0 & 0 & q & p & s & -r \\ 0 & 0 & 0 & r & -s & p & q \\ 0 & 0 & 0 & s & r & -q & p \end{array}\right|} \cdot \underset{T_2}{\left|\begin{array}{ccccccc} x & -y & -z & -w & 0 & 0 & 0 \\ y & x & w & -z & 0 & 0 & 0 \\ z & -w & x & y & 0 & 0 & 0 \\ w & z & -y & x & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 & 1 & 0 & 0 \\ 0 & 0 & 0 & 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & 0 & 0 & 0 & 1 \end{array}\right|}$$

$$= \underset{T_1T_2=T}{\left|\begin{array}{ccccccc} x & -y & -z & -w & 0 & 0 & 0 \\ y & x & w & -z & 0 & 0 & 0 \\ z & -w & x & y & 0 & 0 & 0 \\ pw & pz & -py & px & -q & -r & -s \\ qw & qz & -qy & qx & p & s & -r \\ rw & rz & -ry & rx & -s & p & q \\ sw & sz & -sy & sx & r & -q & p \end{array}\right|}$$

number of dimensions. Thus when $r > 7$, three or more transformations will be required. The first of these rotates the last four axes; in the second, the axes involved are the 4th to the 7th, counting from the rth backwards; and so on. It will be seen that the 4th last axis is rotated by both the first and second transformations, the 7th last by both the second and third, and so on. But if $(r-1)/3$ is not an integral number, there will be an overlap of 2 or 3 axes between the last and the second last of the transformations. If $r/3$ is an integral number, the work may be simplified by writing the last transformation in the form shown in Table V. Finally, if $(r+1)/3$ is an integral number, the last transformation may take the form shown in Table VI.

Table V

$$\left|\begin{array}{ccccccc} x & -y & -z & 0 & 0 & \dots & 0 \\ y & x & 0 & -z & 0 & \dots & 0 \\ z & 0 & x & y & 0 & \dots & 0 \\ 0 & z & -y & x & 0 & \dots & 0 \\ 0 & 0 & 0 & 0 & 1 & \dots & 0 \\ \dots & \dots & \dots & \dots & \dots & 1 & \dots \\ 0 & 0 & 0 & 0 & 0 & \dots & 1 \end{array}\right|$$

Table VI

$$\left|\begin{array}{ccccc} x & -y & 0 & \dots & 0 \\ y & x & 0 & \dots & 0 \\ 0 & 0 & 1 & \dots & 0 \\ \dots & \dots & \dots & 1 & \dots \\ 0 & 0 & 0 & \dots & 1 \end{array}\right|$$

The matrix M was multiplied by a transformation of the type shown in Table III. Having thus obtained an orthogonal matrix in which the first column represents the loads of the provisionally selected first factor, we plotted all the axes against one another in pairs. A number of rotations were tried out. Those which were retained are as follows:

(1) The second and fourth axes were rotated through a negative angle

of $\cos^{-1} 0{\cdot}939$. In this way the second axis was made to correspond closely to Webb's factor '*w*', traits 33 and 34 being given maximum weight at the positive end, and trait 2 maximum weight at the negative end.

(2) On considering the third axis along with the new fourth axis, we found that if trait 4 were given maximum weight at the negative end of the third axis, all the loadings on the fourth axis became positive or zero, which suggested that the fourth primary quality might be unipolar. To obtain this result we rotated the axes through a positive angle of 45°.

(3) The new second axis was taken together with the first axis, and rotated through a negative angle of $\cos^{-1} 0{\cdot}875$. This passes the second axis through the point standing for trait 2, giving it a maximum negative load. The justification for this procedure is that trait 2, *quick oscillation of mood*, is presumably relatively simple. It also has the merit of treating traits 33 and 34, *tendency not to abandon tasks*, etc., as more elementary than traits 20 and 21, *trustworthiness* and *conscientiousness*.

Table VII. *Transformation matrix* T

0·063	0·749	0·556	0·354
0·921	0·004	−0·282	0·268
0·186	−0·632	0·738	0·147
−0·336	−0·197	−0·259	0·884

The final transformation matrix, giving effect to all these steps, is presented in Table VII. Multiplying the centroid matrix M by the transformation matrix T, we get the factorial matrix F, shown in Table VIII.

Table VIII. *Factorial matrix* F

	I	II	III	IV	h^2
1	0·720	0·397	0·000	0·000	0·676
2	−0·007	−0·706	−0·025	0·067	0·504
3	−0·382	−0·569	0·066	−0·002	0·474
4	0·118	−0·465	−0·824	−0·110	0·921
5	0·257	0·401	0·502	0·215	0·525
6	0·042	−0·423	−0·646	0·157	0·623
8	0·792	0·178	0·016	0·108	0·671
11	0·294	−0·333	−0·571	−0·101	0·534
15	0·801	−0·179	−0·310	0·189	0·805
17	0·345	0·075	0·240	0·456	0·390
18	0·052	0·487	0·467	0·590	0·806
19	0·456	0·126	−0·037	0·659	0·659
20	−0·258	0·599	0·313	0·512	0·785
21	−0·381	0·525	0·346	0·501	0·792
23	−0·118	−0·059	0·497	0·182	0·298
27	0·329	0·154	0·505	0·261	0·455
31	0·430	−0·042	−0·292	0·287	0·354
33	−0·009	0·786	−0·158	0·086	0·650
34	−0·232	0·705	0·009	0·121	0·566

(4) *Conclusions as to qualities isolated*

The four primary qualities thus isolated may now be described. In view of the large amount of error necessarily incorporated in such a matrix, it seems unwise to treat as significant any factor loading of less than 0·2, or to use any trait with a load of much less than 0·4 for the purpose of describing and defining a primary quality.

At the positive end of the first axis, traits 15, 8 and 1, viz. *fondness for large social gatherings*, *degree of sense of humour*, and *general tendency to be cheerful*, have loads between 0·7 and 0·8; traits 19 and 31, *degree of corporate spirit*, and *bodily activity in pursuit of pleasure*, have loads of over 0·4; whereas at the negative end traits 3 and 21, *occasional extreme depression* and *conscientiousness*, have loads of 0·38. This factor is obviously akin to Cattell's Surgency-Desurgency, although it may not be absolutely identical with it. It can perhaps best be described by the antithesis between light-heartedness and serious-mindedness.

The second factor is heavily determined. On the positive side traits 33 and 34, *tendency not to abandon tasks*, etc., have loads of 0·7 or over. Traits 20 and 21, *trustworthiness*, and *conscientiousness*, have loads over 0·5, with trait 18, *tendency to do kindnesses on principle*, not far behind. Traits 5 and 1, *readiness to recover from anger*, and *tendency to be cheerful*, also have significant loads. On the negative side trait 2, *quick oscillation of mood*, has a load of over 0·7. Traits 3, *liability to extreme depression*, 4, *readiness to become angry*, and 6, *occasional extreme anger*, are all in excess of 0·4. *Eagerness for admiration*, with a load of 0·333, is probably also significant. This factor differs from Webb's '*w*' chiefly by giving more weight to steadiness, continuity and perseverance than to action from principle or purpose. Possibly the most appropriate name for it at present is Perseverance.

These first two factors are modifications of old friends. Our third and fourth factors are relatively new in psychological analysis.

The third factor is defined at the positive end by four traits with loads in the neighbourhood of 0·5. These are 27, *tact*, 5, *recovery from anger*, 23, *readiness to accept the sentiments of one's associates*, and 18, *kindness on principle*. *Conscientiousness* and *trustworthiness* lie between 0·3 and 0·4, and trait 17, *impulsive kindness*, with a load of 0·24, is also significant. At the negative end trait 4, *readiness to become angry*, has a load of over 0·8, and traits 6, *occasional extreme anger*, and 11, *eagerness for admiration*, are in the neighbourhood of 0·6. The loads of trait 15, *fondness for large social gatherings*, and trait 31, *bodily activity in pursuit of pleasure*, are probably also significant. We have not met with an account of this quality in recent psychological literature, but something reasonably like it is described in I Corinthians xiii. 4–7. There it is called Charity, and we may let the name stand.

As already indicated, the fourth quality is unipolar. It is defined mainly by five traits, whose loads range from 0·659 to 0·456. These are 19, *degree of corporate spirit*, 18, *tendency to do kindnesses on principle*, 20, *trustworthiness*, 21, *conscientiousness*, and 17, *impulsive kindness*. This factor may be called Social Sensitiveness, which is not to be confused with Sociability, or anything resembling a herd instinct. This is clear from the fact that *fondness for large social gatherings* has a factor loading of less than 0·2, and that *eagerness for admiration* has an insignificant negative load.

Table IX shows the percentage of the variance of each trait attributable to the four common factors, and in the final column the percentage of the variance due to specific factors, including chance error. These factors are special only in the sense that they cannot be analysed by means of the data before us. If other traits are taken into account, further common factors may appear, and a fuller analysis will be possible. The need for further investigation is clearly indicated by the large values that u^2 has for some of the traits, notably 23, 31 and 17, where the unique variance is over 60%.

Table IX. *Percentage of variance attributable to common factors*

	I	II	III	IV	h^2	u^2
1	52	16	00	00	68	32
2	00	−50	00	00	50	50
3	−15	−32	00	00	47	53
4	01	−22	−68	−01	92	08
5	07	16	25	05	53	47
6	00	−18	−42	02	62	38
8	63	03	00	01	67	33
11	09	−11	−33	−01	53	47
15	64	−03	−10	04	81	19
17	12	00	06	21	39	61
18	00	24	22	35	81	19
19	21	02	00	43	66	34
20	−07	36	10	26	79	21
21	−15	28	12	25	79	21
23	−01	00	25	03	30	70
27	11	02	26	07	46	54
31	18	00	−09	08	35	65
33	00	62	−02	00	65	35
34	−05	50	00	01	57	43

The values of h^2 are copied from Table VIII, the occasional discrepancies between these values and the sum of the entries in columns I, II, III and IV above being due to the rounding off of numbers. The negative signs are included merely to show the direction in which the factors operate. They are, of course, neglected in calculating h^2.

In making this analysis we have been guided as much by psychological as by mathematical considerations. For example, it has been urged that it is advisable to aim at a solution that is mathematically unique, and that such a solution can be obtained by maximizing the number of zero entries in F. But a solution of this kind depends on the way in which the traits have been selected, and unless they are very numerous, and cover the whole field in an impartial fashion, no great importance can be attached to their actual distribution in the common factor space. Of course considerations of the kind in question have not been altogether neglected, and it will be seen from Table IX that in each of the four columns there is a reasonable number of entries in the neighbourhood of zero. But on the other hand, in determining the position of the axes we have given more weight to psychological probability. A reference to Fig. 2 will illustrate the point. If the axes are rotated through a positive angle of 48°,

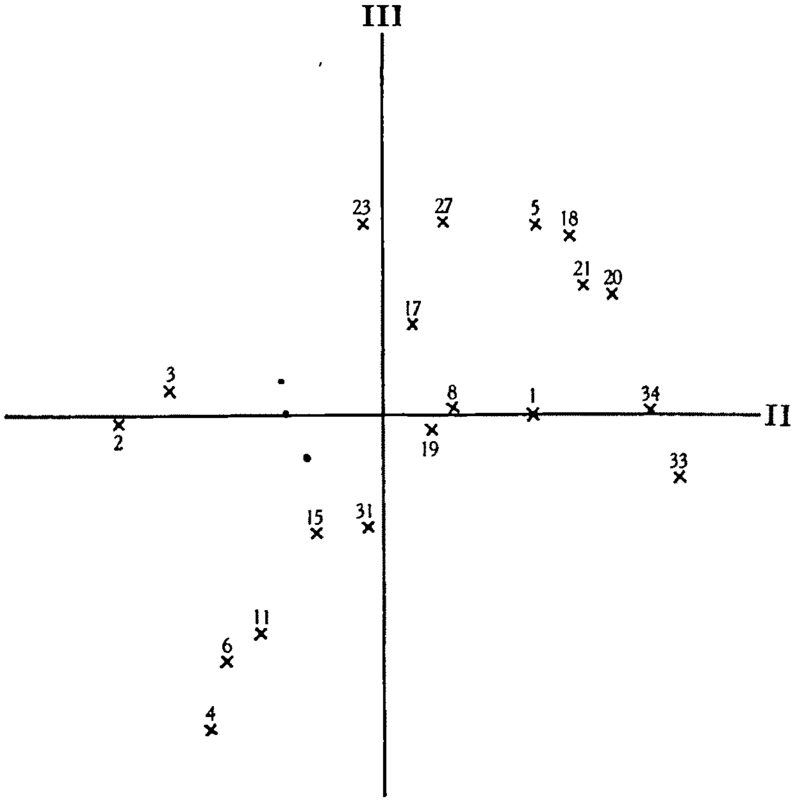

Fig. 2. Plot of columns II and III of F.

the loads of traits 4, 6 and 11 at the one end, and of traits 5, 18, 20 and 21 at the other end of the new second axis will be maximized, while their loads on the new third axis will be reduced to negligible quantities. This simplification, however, is gained at the expense of an increased complexity in traits 1, 2, 3, 33 and 34. We are thus faced with the choice between two analyses, which place the complexity with regard to these two factors in different groups of traits. We consider that traits 20 and 21 are more likely to be complex than 33 and 34, and that trait 2 has a greater claim to simplicity than trait 4. This has decided the position of the axes for us.

The analysis as it stands seems internally coherent, but there are gaps in it. For example, in Fig. 2 the second and fourth quadrants are virtually empty. But it is not difficult to suggest what qualities would help to fill them. Mr Pliable, lacking in '*w*' but probably endowed with some Charity, would almost certainly be found in the second quadrant; and over against him in the fourth quadrant we should expect to locate the obstinate, irritable, cantankerous fellow, along with his even more objectionable neighbour who vindictively nurses a grievance. It would appear, however, that these qualities did not flourish in the groups that Webb studied, and for analysis of them we must turn elsewhere.

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TWELFTH INTERNATIONAL CONGRESS OF PSYCHOLOGY

Preliminary Notice

The Twelfth International Congress of Psychology will be held in Edinburgh, Scotland, from 22 to 27 July 1940. The Honorary President of the Congress will be the Rt Hon. John Colville, M.P., Secretary of State for Scotland.

The Congress fee has been fixed at thirty shillings (£1. 10*s.*) sterling for active members and fifteen shillings (15*s.*) sterling for associates.

Arrangements are being made by which a large proportion of the members can be accommodated in University hostels. The inclusive charge for such accommodation will be about fifty shillings (£2. 10*s.*), excluding midday lunches, arrangements for which will be made elsewhere. Members who do not desire accommodation in the hostels can easily secure accommodation in hotels and private hotels, of which Edinburgh has a large number.

When the Committee of Organization is fully constituted, formal invitations will be issued. It will, however, greatly facilitate arrangements if as many as possible will let the General Secretary know now that their attendance at the Congress is probable. In the meantime subjects for symposia, general discussion and lectures are under consideration by a Preliminary Arrangements Committee.

A volume of Proceedings of the Congress, containing abstracts of papers read, will be published, the cost of which is included in the Congress fee for active members.

James Drever, *President of Committee of Organization.*

Godfrey Thomson, *General Secretary.*

(Moray House, Edinburgh 8)

PUBLICATIONS RECENTLY RECEIVED

The Factorial Analysis of Human Ability. By G. H. Thomson. University of London Press. 1939. Pp. xv+326. 16*s*. net.

Since the pioneer work of Professor Charles Spearman in 1904 a tremendous amount of work has been carried out on the factorial analysis of psychological test scores and of all that has been written on this subject a great deal has been controversial. One of the foremost critics of the Spearman school of analysis was Professor Godfrey H. Thomson, the author of the book under review, and although the points at issue cannot yet be regarded as settled, the statement of the present position made in Professor Thomson's book will be of great value both to those who are attracted by the mathematical approach to psychology and also to those who regard it with suspicion.

Factorial analysis may be considered from three points of view: first, there is the question of the mathematical validity of the various methods of analysis in present use; secondly, there is the difficulty of the actual mathematical calculations involved; and thirdly, and of paramount importance to the psychologist, there is the question as to whether factors isolated mathematically have any psychological existence or can indeed be of any use to the psychologist either theoretically or practically.

As regards the theoretical nature and mathematical validity of the various methods of factorial analysis, only an advanced mathematician is capable of appreciating and judging and the non-mathematical psychologist must perforce accept the ruling of the mathematicians on this point. Professor Thomson deals with this question in a manner calculated to satisfy the advanced mathematician and to instruct the person of average mathematical ability. To the psychologist the interesting fact is made quite clear that there is as yet no unique method of factorizing a set of test scores but that in each of the methods at present in use resort is made to some assumption enabling factors to be estimated or to some device such as "purifying" the battery of tests by rejecting tests which do not yield correlation coefficients which fit into some hierarchical matrix. The psychologist who intends to use some particular method of factorial analysis should therefore ascertain precisely what assumptions he is making therein; he will find these clearly stated in this book.

Turning to the matter of the actual calculations involved in factorial analysis we are greeted in the preface of the book with the statement that "this book has been written so that it can, it is hoped, be read by those who have no mathematics beyond the usual secondary school knowledge." Now it is true that the examples of calculations given in the book involve only elementary arithmetic and algebra, but at the same time the reader must be capable of sustained mathematical thinking in order to make any headway at all with the subject. The mathematical methods used are not such as are normally included in a secondary school curriculum and the reader must be completely familiar with the meaning of such terms as "variance" and "correlation". Moreover, the author himself says that the serious student should have some knowledge of matrix algebra and that is a branch of mathematics which is rarely dealt with even in a university mathematics course. One may conclude, therefore, that whilst the person of average mathematical ability might slavishly follow the arithmetical examples provided and even copy them using his own data, yet only one capable of clear and sustained mathematical thought would be able to understand what he was doing at all stages of the analysis. Such a one will, however, find in this book adequate and clear explanations of the analytic procedures adopted by Spearman, Thurstone, Hotelling, Burt and others, with actual worked examples and accounts of the various "short-cut" methods which have been devised.

The third aspect of factorial analysis, the question of the psychological meaning of the factors emerging from mathematical manipulation, is one which has the chief interest for the psychologist. Professor Thomson has a good deal to say on this point. Perhaps the most significant statement he makes may be quoted in full. He writes

(p. 267): "In brief, then, the author's attitude is that he does not believe in factors if any degree of real existence is attributed to them; but that, of course, he recognizes that any set of correlated human abilities can always be described mathematically by a number of variables or "factors", *and that in many ways*, among which no doubt some will be more useful or more elegant or more sparing of unnecessary hypotheses. But the mind is very much more complex, and also very much more of an integrated whole, than any naive interpretation of any one mathematical analysis might lead a reader to suppose. Far from being divided up into "unitary factors", the mind is a rich, comparatively undifferentiated complex of innumerable influences—on the physiological side an intricate network of possibilities of intercommunications. Factors are fluid descriptive mathematical coefficients, changing both with the tests used and with the samples of persons, unless we take refuge in sheer definition based upon psychological judgment, which definition would have to specify the particular battery of tests, and the sample of persons, as well as the method of analysis, in order to fix any factor." Coming from the pen of Professor Thomson, the passage quoted is extremely significant and should make the psychologist very cautious in accepting and interpreting any mathematical factors which he may isolate. There is a grave danger, as is pointed out in this book, of reifying mathematical factors—a danger which, though usually avoided by the leading exponents of factorial analysis, is frequently positively courted by their enthusiastic disciples. Of what use then is factorial analysis to the psychologist? the reader will ask. It is rather difficult to find an answer from this book except that the author is sceptical of the utility and pessimistic of the future of factor analysis. He concludes, however, that the factor theory will still continue in spite of all its dangers and imperfections, partly on account of its intrinsic interest and partly because it has undeniably been a guide and spur to many investigators.

By examining and explaining *in extenso* the dangers and imperfections inherent in factorial analysis Professor Thomson has rendered a great service to the psychological investigator who has neither the time to study the vast body of original documents on the subject nor the mathematical ability to criticise and appraise the methods for himself. But the book is not easy reading for the non-mathematician.

E. G. O.

American Psychology before William James. By JAY WHARTON FAY. Rutgers University Press. 1939. Pp. x+240. $3.00.

As its title indicates, this book is of historical interest, and, on the whole, of minor historical interest only. Miss Fay has no difficulty in showing that both American writers and others have neglected American psychology before William James, but it seems likely that the perusal of her interesting and brightly written pages will leave most readers still of the same opinion that very little of original and striking character was achieved in America during the long period with which her book is concerned. She divides the whole period into three: the period of theology and moral philosophy (1640–76); that of intellectual philosophy (1776–1827), and that of British and German influences (1861–90). The period from 1827–61 is hailed as "the era of American textbooks": 1861 seems rather an early terminal point to set for this. On the whole she has done her work well, and must have read a vast quantity of literature, most of it dull. But the impression remains that until William James brought his genius into the field of psychology, the American contributions were derivative and on the whole more pretentious than helpful.

English, and especially Cambridge readers (possibly provided they do not belong to the college mentioned) will be delighted with an early paragraph: "The Puritans made prompt preparations for elementary education, and the need for preparation of ministers of the Gospel led to the founding of the first college, modelled after Magdalene at Cambridge, a Puritan institution in which many of the New England colonists, including John Harvard, had studied." But this is not a fair sample of the author's accuracy.

Logical Aspects of Educational Measurement. By B. OTHANEL SMITH. New York: Columbia University Press. In England, Oxford University Press: Humphrey Milford, 1938. Pp. x+182. 12*s.* 6*d.*

The main thesis of this book is to define what is meant by measurement, to enunciate the essential principles on which the construction of measuring instruments must be based, and to examine critically the question of whether learning has the property of being measurable and, if so, whether the instruments that have been used for its measurement are valid. The author states that measurement is the quantitative evaluation of a property by means of an instrument which is constructed in accordance with certain general principles, and that to be completely measurable a property must be capable of being described as more or less in amount, it must be such that a property of like kind can be found equal to it, and it must be capable of addition. The author denies that equal units have been established in measuring educational attainment and shows that the many measuring instruments which have been used in educational and psychological measurement do not satisfy the axioms of addition. It follows, therefore, that learning and educational attainment cannot be completely measured but only partially measured in a manner similar to the measurement of temperature by means of a thermometer.

The author forecasts that the future of educational measurement will be in two directions: "First, it will seek for those independent and homogeneous elements with which it can deal most successfully; and second, it will rest content with the degree of accuracy it has attained and will attempt to adapt its techniques and procedures to the requirements of qualitative evaluation." The problem discussed in this book is one which is of importance to the increasing number of psychologists who rely on test scores and statistics for the basis of their theories. The exposition is reasonably clear but the reader is irritated by innumerable footnotes, most of which could be avoided by the usual system of reference to the comprehensive bibliography at the end of the book.

Psychophysiologie der Tastsinne. By EMIL V. SKRAMLIK. Leipzig: Akademische Verlagsanstalt. 1937. Pp. xiii+935, 308 illustrations. R.M. 78.

The book attempts to give a comprehensive survey of the facts and theories of the tactual senses, the discussion being centred about the senses of pressure, temperature, pain and kinaesthesis, and the cooperation of these four senses with one another. A particularly detailed treatment is accorded to those fields in which experimental contributions have been made by the author himself and by his pupils. Although the amount of literature covered is tremendous, the work cannot be considered as completely exhaustive, as I was able to determine, for instance, when I checked the discussion of the problems of vibratory sensivity. Wherever specific psychological problems are touched upon, one feels that the treatment does not do justice to the "totality" point of view which nowadays receives, and deserves, so much emphasis. Nevertheless, the psychologist owes the author a debt of gratitude for having undertaken the gigantic task of presenting the facts of tactual sensitivity in comprehensive form. Every future worker in this field will find it advisable to go to Skramlik for purposes of orientation.

DAVID KATZ.

Abnormal Speech. By E. J. BOOME, H. M. S. BAINES and D. G. HARRIES. London: Methuen. 1939. Pp. xiv+162. 6*s.* net.

This book should be of considerable use, especially to the practising physician, provided it is read with care and is not regarded as a complete and final treatment of the subject. It contains a very useful classification of disorders of speech, both organic and psychogenic, although some of it, particularly the classification of aphasic and allied defects, seems somewhat out of date. Further, it is distinctly practical in bent and indicates plenty of exercises, most of them excellent and clearly described, for improving utterance. On the whole the least satisfactory section concerns stammering and stuttering, because it is suggested that the basic successful treatment is simply by relaxation. The authors do, indeed, take care to say that this treatment is essentially an early device, and that the defects may, if they are at all long standing, be complicated by many psychological difficulties which render treatment hard and recovery far more prolonged. But this is not emphasized sufficiently, and there are plenty of cases, even in the early stages, where the relaxation recommended will be found very hard to obtain unless special measures are taken. Still, if it is read carefully, and checked against actual experience, the book should do a lot of good. It contains much information concisely arranged, and some interesting statistics, as well as an abundance of suggested practical exercises.

Mental Tests. By FRANK N. FREEMAN. London: G. Harrap. 1939. Pp. xi+460. 10*s*. 6*d*. net.

Professor Freeman's text-book is already well-known and has been found widely useful. It has been thoroughly revised and brought up to date. It still remains in the main a factual book, concerned especially to describe and to illustrate a wide variety of tests, to give sufficient information about common methods of marking and scoring, and very briefly to characterize the main theories in the field. It is an extremely efficient piece of work, and should be of wide use in training college and university courses.

The Dream World. By R. L. MÉGROZ. London: The Bodley Head. 1939. Pp. xiv+319. 10*s*. 6*d*. net.

Here is a most excellent book in which to dip and read. It could almost be called Dreams with Notes, for although it has an introduction and a historical commentary on theories, it is mainly full of dream descriptions and these are gathered both from famous and from unknown people. Very queer and interesting many of these dreams are, and the psychologist who pleases can read anywhere and interpret as he likes, or if he likes better he need not interpret at all, but can simply enjoy. As a matter of fact the author writes with learning and with skill and his book is much more than a mere compilation. The book is all the better because, though it is clear that the author knows a lot about his subject, he has most skilfully avoided all elaborate and technical treatment.

Conscious Orientation. By J. H. VAN DER HOOP. London: Kegan Paul, Trubner, Trench and Co., Ltd. 1939. Pp. x+352. 15*s*. net.

There is little doubt of the difficulty in arriving at some adequate method of describing and classifying qualitative differences in personality; and the concept of types seems in many ways to offer a useful way of surmounting this difficulty. Jung's dual, extravert-introvert, classification has always been one of the most popular of these typologies; but his further classification into sensation, feeling, intuition and thinking types is much harder to grasp, and it has been adopted by few other psychologists. Dr Van der Hoop, however, finds that with suitable modification it may usefully be employed in the understanding and description of different types of personality as the latter appears in its conscious orientation towards the environment, both in the normal individual, and also in the psychotic and neurotic. But he substitutes for the sensation type, the instinctive type—comprising those who react immediately not merely in response to their sensations, but also in accordance with their instinctual and emotional needs. The feeling type is guided by more consistent and enduring affective attitudes, in the nature of McDougall's sentiments, in which a variety of emotional reactions are combined. Characteristic differences are observed according as the orientation of the personality is introverted or extraverted.

Dr Van der Hoop then demonstrates how such an eight-fold classification may be used to gain an understanding of the structure of the personality which is particularly useful to the psycho-pathologist. In the neurotic, this structure conforms to those of the normal type, and determines to a great extent the nature of the abnormal behaviour set up by some excessive environmental strain of the kind dealt with by Freud. A better understanding of the psychotic is obtained, however, if some abnormal deviation is recognized from the structural type.

It seems probable that this typological scheme may assist both the psychologist and the psycho-pathologist in the understanding of those qualitative differences in the structure of personality which are so baffling and so obstructive to the assessment and description of individual personality. As to the ultimate reality and the philosophical validity of his scheme, we must for the present remain doubtful.

Mis-Mated: The Psychology of the Unhappy Marriage. By JOHN F. PETERS. London: John Bale Sons and Curnow Ltd. 1938. Pp. 213. 7*s*. 6*d*. net.

"Are you aware", ask the publishers, "that your position in your family, in relation to your brothers and sisters, determines your chances of making a happy marriage? Are you aware that if (for example) you are an only son without a younger sister, you will be unhappy if you marry a woman with elder brothers, etc.?" This perhaps sufficiently indicates the topic of the book, which is written with immense high spirits, tremendous confidence, and much illustration, by a man who many times points out that he is an only son and hence an introvert, and who obviously has a great interest in other people. No brief summary is possible. The whole rests upon the complete acceptance of the introvert-extravert classification of human beings—together with whatever border types are convenient. Introversion or extraversion are asserted to be determined early by position in the family, and whether a person is happy in marriage or not depends on his or her finding a mate who has the right relation of introversion or extraversion to himself or herself. Hence the great thing to do is not to fall in love until one has discovered how many brother and sisters the potential loved one had, and how they were spaced out. There is a lot of ingenious illustration drawn from a study of the married happiness or unhappiness of famous people, and from more or less analogous evidence.

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VOLUME XXX JANUARY, 1940 PART 3

OBITUARY NOTICE

SIGMUND FREUD, 1856–1939

WITH the death of Sigmund Freud on 23 September 1939, at the age of 83, one of the great figures of psychology has passed away—we can truly add, one of the great figures of our own time, for Freud has undoubtedly exercised a wider influence upon his generation than any other psychologist. He effectually cleared psychology from the charge of being academic, of being (as H. G. Wells, one of Freud's personal friends in his last days, once put it) "a happy refuge for the lazy industry of pedants". Freud's work made it clear that psychology really had something of importance to say of human life as actually lived from day to day, of men's problems and worries, of their loves and hates, their heroisms and their follies, of the heights and depths of their innermost aspirations and desires. If much of his message was unwelcome, it was nevertheless vivid and compelling, and altogether free of that quality of abstraction and remoteness from ordinary life that seemed to the plain man to be characteristic alike of the philosopher's study and the experimentalist's laboratory: and hence it is that the name of Freud is to-day almost certainly better known throughout the world than that of any other worker, past or present, in the field of mental science.

As a thinker and observer, Freud was at once intensely original, immensely assiduous, courageous and self-confident in a way that was compatible with calm and penetrating self-criticism, and endowed with an astonishing breadth of vision that enabled him to realize the full implications of his own discoveries.

His originality was shown not only in the multitude of instances in which he brought new insight to the elucidation of the obscurer forms of human thought and motivation, but also in his whole attitude towards the problems of psychology. Although many before him had found a place for the "unconscious", "subconscious", or some similar concept, in their systems, either on general philosophical grounds or as needed for the explanation of certain unusual or pathological phenomena, Freud was the first to make such a concept the really essential element in a psychology far-reaching in its scope but fundamentally empirical in character, built up as it was on the basis of his own painstaking observations over many years. His use of original intuition, both for seizing on significant facts and for viewing them in meaningful perspective, has certainly never been surpassed, and seldom if ever approached, in the whole history of psychology. In this respect, however, he was, in his own exceptional degree, true to the great German tradition, which (as has been well brought out in recent discussions in this country) has always attached importance to happy insight and ingenious speculation and has been little attracted by the mere accumulation of facts and measurements for their own sake which has sometimes found favour in America. Facts there are of course in plenty in Freud's writings, but they have mostly been used, almost as soon as obtained, for the purpose of some suggestive addition to, or re-orientation of, psychological theory.

Freud's assiduity is manifested eloquently in the great number of his books and papers. The list of his writings up to 1932 occupies 14 pages of Murchison's *Psychological Register*, and (excluding translations) contains 225 items, of which some 20 represent the predominantly neurological interests of his pre-psychoanalytic period. His *Gesammelte Schriften* in their forthcoming final edition will run to sixteen imposing volumes. When we remember the pregnant and condensed nature of many of these contributions and also the fact that they were written by one whose working hours were largely occupied with a novel and very time-consuming form of clinical practice, we cannot but be impressed by the mere quantity of his literary output, quite apart from its value and originality.

What we have ventured to call Freud's courage and self-confidence are revealed in many aspects of his life and work. The fact that he was able to persist for many years in his chosen line of investigation in spite of disapproval or neglect from scientific colleagues, and to contemplate with equanimity the likelihood that his work would never be appreciated in his own lifetime, finds of course a parallel in the career of many others who have made signal contributions to knowledge (and indeed it may be said that Freud had the consolation, denied to them, of knowing something of the reason why his own work encountered rebuff or incredulity). More specially characteristic of him, however, was his ability to trust to the path of psychology and not desert it even when it appeared to have misled him. The classical instance of this occurred on his discovery that many of the accounts of sexual seduction in early years which he heard from his patients were no more than figments of these patients' imaginations. Instead of being led to a fundamental scepticism concerning the value of what his patients told him, and perhaps to an equally fundamental distrust of his own judgement, he realized that what was here important for his own purpose was, not so much the distortion of objective fact as the widespread existence of the phantasy in question—the psychological reality rather than the physical one. His belief in psychic determinism, which had been throughout one of the essential postulates of his thinking and had led him to make such extensive use of the concept of the "unconscious", here stood him in good stead, and allowed him to see what most others would assuredly have missed. In this and other matters, however, he did not hesitate to acknowledge and correct his own errors as soon as they became apparent to him—at whatever cost to his theoretical assumptions (as when he felt himself compelled to adopt the concept of Narcissism, thus enlarging still further the immense and by no means popular role he had assigned to the *libido*, and obliterating the neat theoretical distinction between this and the ego-trends), or to his reputation and esteem (as when in later years he expressed astonishment that he could so long have neglected the role of aggression in human life). But in recognizing the limitations of his earlier views, he was careful to avoid losing touch with the solid ground he had already won. Indeed, he had a very clear realization of the importance of his own work, and it was he himself who instituted the comparison (with regard to their effect on human complacency) between his discoveries and those of Copernicus and Darwin—a comparison which hostile critics did not omit to stigmatize as a sign of his immodesty.

Most of all perhaps was his courage shown in his resolute facing of the less agreeable aspects of reality, whether in his own patients, in human disposition generally, in the limitations of our psychological knowledge and our therapeutic power, or in the very nature of life itself and its bearing upon human destiny. Certainly the author of *The Future of an Illusion* and *Civilization and its Discontents* was himself free (as many felt, painfully and embarrassingly free) from any illusion as to the existence of Utopia, whether in the past, in the present, in another world, or in an age to come. It seems likely indeed that this aspect of his character was an essential condition of his making the discoveries he did, of his being able to penetrate the veil which, on his own showing, protects us from the disturbing knowledge of our unconscious tendencies. But Freud differed also from the ordinary pessimist or breaker of unwelcome news, in that he availed himself of none of the compensations to which the latter is wont to resort. He appears to have derived but little satisfaction from the shocks he gave to men's respectability and equanimity. This manifestation of sadism, one which is assuredly a temptation to the psycho-analyst, as to all who have incurred moral ostracism or broken cherished illusions, he seems to have held in remarkable control, when we bear in mind the unique opportunities he possessed in this direction. Indeed the two essays written in reference to the Great War (which can profitably be re-read at the present time) evidence a certain tender yet intensely virile sympathy, which is entirely free, alike from the usual wartime bias and enthusiasms, and from any vulgar self-justification in the spirit of "I told you so" in which a lesser mind might have been tempted to indulge. He never became bitter or cynical, nor did he (as some distinguished pessimists have done) utter mocking taunts to man upon his folly or defiant protests to the universe on its indifference or its cruelty. There is something which can only be called heroic in Freud's attitude towards suffering and evil, an attitude without rancour, lamentation or complaint. One is tempted to regard him, the bringer of great gifts to mankind, as a modern Prometheus—a noble figure such as the Greek tragedians would have lovingly depicted. But perhaps it is better to note the impression that this aspect of his thought and character has made upon an eminent contemporary. Speaking of the "pessimistic philosophy of life" set forth in *Beyond the Pleasure Principle*, Sir Percy Nunn says that he there "outlines a conception so tragically grand that it extorts admiration, yet so terrible that it seems to make even his most faithful followers uneasy".[1]

A fourth element of great importance in the development of Freud's thought was his breadth of outlook and capacity for seeing resemblances in matters which had hitherto been regarded as remote and unconnected. It is principally owing to this characteristic that psycho-analysis started as a therapeutic method for dealing with neurosis and ended by becoming a discipline that has affected almost every branch of knowledge that deals with the life and works of man. The general history of psycho-analysis is too well known to need repeating here. It is not necessary to remind readers of this Journal how Freud was able to apply what he had learnt in his consulting room to dreams (which he himself always regarded as one of the most important and assured of all his conquests), to what he called the "psycho-pathology of everyday life", to child psychology (another vastly important field), to wit, humour, art, literature, history, social psychology and anthropology. We will confine ourselves to recalling two examples of this ability to detect unsuspected resemblances. The first is the parallel he drew between the "pre-genital" development of the young child and the behaviour of the adult sexual "pervert", a comparison which underlies the epoch-making *Three Contributions to the Theory of Sex*, itself the basis of much of that great increase of knowledge in the sexual sphere that we owe to psycho-analysis. The second example consists in the remarkable series of resemblances he pointed out between the neurotic mind, the child mind and the mind of primitive man (his much discussed theory of totemism will serve as a specific instance). Before the appearance of *Totem and Taboo,* abnormal psychology, child psychology and cultural anthropology had been distinct disciplines with few if any points of contact. By bringing them into relation with one another, Freud fructified them all, and laid the foundation of a new comparative psychology of the human mind, the full consequences of which are only now beginning to appear, with the advent of a new generation of workers (as yet unfortunately but few in number) with sufficient training to apply the knowledge gained in adjacent fields to the special problems of their own.

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[1] In the Preface to Barbara Low's *The Unconscious in Action*, 1928.

To the present writer it has always seemed that Freud's scientific work in the field of psycho-analysis may conveniently be considered as falling into two main periods. In the first, extending from his collaboration with Breuer till shortly after the end of the war, he was mainly concerned with the fundamental mechanisms of the unconscious and the protean manifestations of the *libido.* It is this part of his work which, up to the present, has received the wider public recognition and has more largely influenced current thought. On the other hand it was with regard to this period also that the charge of "pan-sexualism" has most often and most plausibly been brought against him. Freud, however, was himself all the time conscious of the fact (which he affirmed repeatedly, but not often enough to satisfy his critics) that there were other important aspects of human personality which had as yet scarcely been touched by psycho-analysis.

In *Beyond the Pleasure Principle,* which appeared in 1920, he is seen dramatically groping his way towards the immense problems presented by this larger field, a field that can be regarded as definitely entered with the publication of *The Ego and the Id* in 1923. From that time onwards he was largely occupied with the repressing—as distinct from the repressed—factors, with aggression and with the growth and nature of human morality; which latter, in his hands, soon revealed itself as resulting from the interplay of a highly complex set of forces, many of them obscure and sinister in their origin and implications. Although as yet less generally appreciated, there can be little doubt that this later work has an importance at least equal to his earlier formulations with regard to sex. Freud has shown that man's individual difficulties and his social failures are due, not merely, as moralists throughout the ages seem usually to have thought, to inadequate control over strong a-social impulses, but also to the primitive, chaotic and archaic nature of much of his morality itself, which is often no less unreasonable and unrealistic than the clamourings of instinct, and which is moreover liable to be highly charged with that fatal aggressiveness which, as many are beginning to think, may lead to the downfall of our present culture.

Great as were his adventures in the realm of thought and observation, Freud's external life was for the most part calm and uneventful. Born at Freiberg, Moravia, on 6 May 1856, he lived in Vienna from the age of four till about a year and a half before his death. He attended the University from 1875, obtaining the degree of Dr.med. in 1881. He was Dozent for Neuropathology from 1885 to 1902 and became Professor Extraordinarius in the latter year. It was only in 1920, when he was already world-famous, that he received from his own University the full academic recognition implied by the title of Professor Ordinarius (though actually he never held the position).

In 1885 he studied in Paris with Charcot. In general those who know him best are inclined to think that the influence that the great French psychiatrist exercised on Freud was not decisive. In one matter at least, however, Freud himself suggests that Charcot's words were important for the subsequent development of his own thought (though in this connexion he links Charcot's name with those of Breuer and Chrobak). Speaking of certain symptoms presented by one of his cases, Charcot once remarked privately: "Mais, dans des cas pareils, c'est toujours la chose génitale, toujours—toujours—toujours", and at the same time emphasized his words by jumping up and down "with the vivacity peculiar to him". Freud wondered at the time, why, if Charcot held this view so strongly, he did not mention it in his official lectures. Herein perhaps we may detect a significant difference between the two men, one in which the peculiar courage of Freud, for whom truth was always more important than discretion, surely played its part.

Shortly after his return from Paris he renewed his friendship with Josef Breuer—to whom he already owed some inspiration and who had already evolved the "cathartic" treatment which can be regarded as the fore-runner of psycho-analysis—and began the collaboration that eventually led him, when continuing the work alone, to the development of the psycho-analytic method and all that followed from it. Their joint work, *Studies in Hysteria*, was published in 1895 (though some of the constituent papers had appeared earlier), and this event is usually reckoned the first in the history of psycho-analysis.

From 1902 onwards Freud ceased to work in isolation. A few others, convinced of the truth and importance of his contentions, began to gather round him. The little group enlarged slowly, and in 1908 there was held the first meeting of what was to become the International Psycho-analytical Assocation, Ernest Jones being the earliest and most prominent British representative. This Association (with constituent societies in various countries) has, since its foundation, officially controlled the destinies of the psycho-analytic movement.

In 1909 Freud visited America at the invitation of Stanley Hall and delivered at Clark University the subsequently published *Five Lectures on Psycho-analysis*, receiving at the same time the degree of LL.D of that University. Apart from this he seldom left his native land except for attendances at the congresses of the International Psycho-analytical Association and for fairly frequent visits to Rome, whither he was strongly attracted by his archaeological interests. In 1938 however, on the Nazi occupation of Austria, he was compelled to leave Vienna, the city where he had lived since his early childhood, and the last year and a half of his life were spent in London, where he received a welcome which touched him very deeply. Some time before his arrival he had been made a Foreign Member of the Royal Society; while in 1926 he had accepted Honorary Membership of the British Psychological Society. It was his hope, had he lived longer, to become a British citizen.

In his two writings of an autobiographical nature Freud has told us much about the development of his ideas and the early history of psycho-analysis, but little concerning his own intimate life or his personal feelings on matters unconnected with his work. Where he himself has been so reticent, it would be unseemly at the present moment to try to pierce the veil which he so clearly wished to be drawn over his purely personal affairs. There can be little doubt however that future psychologists and historians of science will display a great and legitimate curiosity as to what manner of man it was who was able to penetrate so much further than others into the depths of the human mind. Those who were privileged to meet him realized at once that he possessed not only a great intellect but a great personality. Of many remarkable characteristics perhaps the most striking was his sincerity—charming, but at the same time (accustomed as we are to more tortuous methods of social intercourse than he employed) startling and disconcerting. He once wrote that one of the inconvenient consequences of his long preoccupation with psycho-analysis was that he found it impossible to tell a lie. When face to face with him one realized that this statement was less outrageous than it might at first appear; one felt inclined to believe also that its converse might also contain an important element of truth, and that it was largely because of his courageous (and inevitably disconcerting) frankness in thought and word that he had been able to become the founder of psycho-analysis.

Freud was a delightful speaker and lecturer. Disdaining the arts of the orator, he yet gripped his audience with his first words, and created a peculiar atmosphere of intimacy in which the hearers felt themselves, as it were, to have been granted the privilege to listen-in to the reflexions of a remarkable mind. The same charm of intimate personal contact pervades many of his writings, both those in which we get the impression that we are watching the master in the very process of wrestling with new and difficult ideas and those admirable didactic expositions in which he himself was on familiar ground and only concerned with making the path smooth for those with lesser knowledge. Added to this, his later works are suffused with that quality of strong yet reposeful mellowness and detachment from the clamorous urgencies of life, that comes characteristically to certain of the greatest minds in their full maturity.

On the astonishing nature and far-reaching import of Freud's contributions to science; psychologists are all agreed. Like others, they have in their time been startled and shocked at many of Freud's contentions, and this bewilderment has sometimes found rationalized expression in the form of scientific or quasi-scientific objections. The day of such rationalizations is approaching its end, but there remain some real and difficult problems with which psycho-analysis still confronts psychologists (and among psychologists we here include psycho-analysts themselves). There are two such problems of a general order, though perhaps they can be more accurately looked upon as two aspects of a single problem. In the first place, how far and in what ways can psycho-analytic discoveries be reconciled with the findings of psychologists of other schools who have worked with other methods? And in the second place, how far do these discoveries admit of rigid proof by the accepted canons of scientific evidence (for it can scarcely be denied that psycho-analysis does in fact raise some knotty questions from the point of view of logic and scientific method)? The two problems tend to be one, inasmuch as other schools are often in possession of more unquestionably valid scientific methods, whereas psycho-analysis often presents us with conclusions which, if true, are clearly of greater practical and theoretic value. (As the matter has sometimes been put, vividly but with evident exaggeration, by those who are sceptical of all psychology, what has been proved by psychologists was not worth proving, while what is important has not yet been proved). The problems have arisen in this form partly no doubt because of the divorce that has always existed and to a considerable extent still exists (though it is less in this country than in some others) between psycho-analysis and every variety of "academic" psychology. A separate origin may have been inevitable; indeed it has often and very plausibly been said that psycho-analysis could never have been invented by anyone brought up, say, in the Wundtian tradition. But a permanent separation is unthinkable, and the sooner and more efficiently the gulf is bridged, the better it will be for psychology as a whole. The process of bridge-building is difficult and likely to tax the ingenuity of psychologists for years to come. Fortunately the work has already begun, and from both sides, though in a humble way; certain psycho-analysts, for instance (notably perhaps Franz Alexander and his colleagues in Chicago), are beginning to pay attention to the planning of controlled and systematic investigations of particular problems, while experimental psychologists on their part are finding that some at least of the Freudian doctrines can be put to the test by adaptations of existing experimental techniques and can in turn help to illumine the data obtained by such techniques.

What modifications Freudian theory may undergo as the result of its fuller incorporation into general psychological thought, it is impossible to tell at present. As a theorist, Freud himself was never over-much concerned about the impregnability or permanence of his theories. He used them, as theories should be used, to bring about a temporary clarification of his thought, confronted as it was by his strange discoveries, and as stepping stones to his further advance. Indeed we receive the impression that he never surveyed his theoretical structure as a whole. This he left for others to do; and, as we know, he would unhesitatingly scrap any item of theory that no longer served its purpose. We know too that such process of scrapping left his essential findings for the most part undisturbed. These findings have been, and doubtless will again be, revealed as incomplete, but none have as yet been shown to be fundamentally unsound or illusory. On the contrary, their value has been proved by the very fact that they have so often led to new and wider findings. Freud himself, though his life was devoted to the investigation of the less reasonable aspects of the human mind, was a firm believer in the ultimate value and power of the still small voice of reason; and the greatest service that psychologists can render to the memory of this great thinker is to use his contributions to their science for further and fuller investigations in the light of reason. Had Freud been granted a

THE PROBLEM OF THE DIRECTION OF THOUGHT

By GEORGE HUMPHREY

Queen's University, Kingston, Canada

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  (2) *Thorndike's associationism* (p. 186).
  (3) *The conditioned reflex theory* (pp. 186–188).
  (4) *The Würzburg school* (p. 188).
III. *Non-associative theories* (pp. 188–194).
  (1) *Dynamic tendencies* (pp. 188–192).
  (2) *The theory of Selz* (pp. 192–193).
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IV. *Conclusion* (pp. 194–195).
  References (pp. 195–196).

I. INTRODUCTION: THE ORDERLY SUCCESSION OF THOUGHTS

WHEN one examines an actual train of thought as it occurs in a human being two primary and distinct problems seem to emerge. There is first of all the problem of what may be called the material of thought. Of what is the train of thought composed? Does it consist of ideas, of images, of imageless thought processes, of Gestalten, or what not? Or, if one prefers to speak in terms of behaviour, should the most highly adaptive human activity be analysed into conditioned reflexes, behavioural Gestalten or into what unit, if any? On the other hand, there is the problem of why one member of the train succeeds another, why a particular idea or image occurs, more or less relevantly to the point at issue, instead of the million and one irrelevancies that might occur. Objectively speaking, in a train of the most highly adaptive behaviour what causes one activity to follow another relevantly to the situation? This is the problem of mechanism. It was Aristotle who first called attention to the fact of the orderly sequence of mental processes. "Recollection occurs", he says, "because processes naturally follow each other in an orderly manner" (1). It is primarily to the problem of the orderly sequence of thinking that this paper will be devoted, although it will appear that the two problems cannot ultimately be separated. In order to discuss mechanism intelligently it is necessary to a certain extent to consider material and vice versa.

II. Associative hypotheses

(1) *Early theories*

On what principle does the orderly succession of thoughts take place? Undoubtedly the most influential hypothesis has been that of association in some form or other. Suggested by Plato, elaborated by Aristotle, annotated through the Middle Ages, the doctrine of association gave its name to the dynasty of philosophers and psychologists known as the British Associationists. From such philosophers as Hobbes, Locke and Hume the psychologists characteristically took over the doctrine, and we find a succession of names such as the Mills, father and son, Bain, Spencer and Lewes, forming a group who may be said to comprise a pre-experimental school of associational psychologists. Thus it came about that the advent of experimental psychology found ready made a hypothesis for the mechanism of thinking. And thus it was, also, that, although there were important dissentients, the associational hypothesis may fairly be said to have been the conventional explanation of the sequence of thoughts when the Würzburg group began to work on the psychology of thinking at the beginning of the present century.

The main hypothesis of the orthodox associational psychology was of course that all other phenomena of consciousness are elaborated from the elementary sensations by means of association ((2), p. 259), association being roughly defined as the process by which, if two experiences have occurred together, the one will tend to reproduce the other at a future time. Two separate hypotheses are really involved; that which sets up the sensation as the sole primary datum and that which sets up association as the sole elaborating mechanism. The discussion of the primacy of the sensation belongs under the heading of the material of thinking, and is thus not specifically relevant to this paper. Two remarks are, however, in order. The first concerns the impressive name of Aristotle. His well-known dictum that there is no thought without image has been quoted in support of the sensationalist argument. For example, Titchener ((3), p. 117) classes Aristotle as a sensationalist. However, the course of Aristotle's argument makes it fairly clear that in this passage the philosopher means exactly what he says and no more. Anybody who reads without prejudice the relevant passages of the *de Anima* may convince himself that Aristotle distinguishes thinking from the images which are utilized in thinking.[1]

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[1] See, for example, *De Anima*, 427*b*: "Imagination (i.e. the formation of images) is different from either perceiving or discursive thinking, though it is not found without sensation, or judgment without it" (trans. Smith). Similarly our concepts "are not images though they necessarily involve them" (*ibid.* 432*a*).

Not that psychology of to-day has to rely on Aristotle to tell us what is the nature of thinking. But if the authority of the name is to be invoked at all, this should be done not in support of the sensationalist hypothesis, that thinking is the elaboration and manipulation of sensation, but rather of the very different one that thinking of a non-sensory nature utilizes experience of a sensory nature. This latter hypothesis cannot properly be classed as sensationalistic.

The other remark concerns the question of analysis. In order to account for actual experience the later associationists had to postulate that association not only binds experiences together, but also transforms them. Thus, for example, we have the younger Mill's 'mental chemistry', by which associated sensations are mutually modified and which substitutes the chemical analogy of the union of molecules for the mechanical one of Mill the elder. Now this is a perfectly legitimate assumption, but it does depart from the original spirit of the associational hypothesis according to which one experience reproduces another. Stout pointed this out forty years ago. In fact, the stricter associational doctrine current at the beginning of the experimental period reverts to the doctrine of reproduction at least by implication. For the phrases, viz. 'associative recall', 'reproductive tendency', and so on, current at the time the Würzburg School began their work thirty-five years ago, do imply that the original experience is reinstated, not remade. The associative psychology, as originally understood, and as adopted by the early experimentalists, is essentially a psychology of the reinstatement of past experience. But we have seen that a psychology of reinstatement had previously been found to be inadequate. That is to say, it was found impossible to account for what actually happens during thinking by the sole assumption that any process of experience has the tendency under specified conditions to reproduce another with which it has been associated.

At the end of his history of the Association psychology Warren sums up the difficulties as follows. "It would appear that the associationists included under the term *association* two or three rather distinct operations.:..To group these three operations under a single name", he continues, "is a verbal simplification scarcely justified by the facts with which we are dealing....Again", he further continues, "the phenomena of *attention* and *discrimination* do not seem to be accounted for under the associationist's treatment. These phenomena appear to involve distinctive operations upon the elementary data. In other words, instead of accounting for all complications of experience by means of one single type of operation, we seem justified in assuming several different operations" ((2), pp. 306–7).

(2) *Thorndike's associationism*

In spite of these difficulties to which attention has been called at many different times it is fair to say that the associational theory is very much alive to-day as part of the background of many able psychologists. Its vitality is shown by the fact that it has survived translation into behaviouristic coordinates. Thus Thorndike's explanation of thinking as due to the operation of bonds shows leanings towards behaviourism. The bonds in terms of which he casts his whole system are ultimately associational. It may here be noticed that, perhaps by historical accident, what may be called objective associationism has taken into account a problem not sufficiently recognized by its subjective counterpart. I refer to the problem of the rejection of error. So much impressed, indeed, were the early comparative psychologists with the fact of the elimination of mistakes that they exalted 'trial and error' into a religion, some indeed still maintaining that it provides a psychological criterion between man and the animals, others that human thought is of an essentially trial and error nature, though on a plane not reached by animals. Here it is hardly necessary to mention that trial and error cannot provide an explanation of the direction of thought, whether animal or human, or whether the discussion is conducted in subjective or objective terms. Trial and error is a matter of observation, something to be explained rather than a principle of explanation. In the total picture, the fact that irrelevant processes are rejected is the counterpart of the fact that more or less relevant processes are initiated. The early animal workers performed a great service by throwing into relief the corrective aspect of directed behaviour; but they did not solve the problem of why one process succeeds another. They rather added or perhaps stressed a complementary set of data, often, in their enthusiasm, losing their vision of the whole picture in a contemplation of that part of it which their own methods had thrown into prominence.

(3) *The conditioned reflex theory*

Another, more stringently objective, type of associationism must now be considered, viz. the well-known conditioned reflex theory. Like their predecessors, the classical associationists, the conditioned reflex enthusiasts claim to be able to analyse the highest human activity into elementary data—the conditioned reflexes—originating in the stimulation of the sense organs, such data, like those of the subjective theory, being elaborated into more complex wholes by means of a single mechanism, that of the process of conditioning. Thus for each of these two associational hypotheses the highest activity, of thought and action respectively, differs only in complexity from the process ensuing upon elementary sense excitation. It will be seen that in his effort to avoid what he considered to be the mysticism of the subjective psychology, Watson succeeded only in repeating the programme of perhaps the most reactionary of the subjective schools. Instead of solving the problem he had merely transformed its coordinates.

Like its predecessor, the conditioned reflex theory is essentially one of reproduction of the past. The conditioned stimulus in Watson's and Pavlov's theory, at least in its earlier form, is attached to, and later reproduces, the response which originally belonged to the unconditioned stimulus. It is interesting to find that the exact measurement made possible by objective registration supplements the criticism already made of the subjective associational doctrine. The earlier associationists had found that a doctrine of reproduction was inadequate to explain the facts. Objective registration demonstrates that at least in the conditioned reflex, which is the point at issue, reproduction does not take place. Examination of the records, both those of Pavlov and of other investigators, show that, in the overwhelming majority of cases, the conditioned reflex is quantitatively different from the unconditioned one, being generally very much the smaller of the two; there is also some evidence that, in certain cases, it is qualitatively different. Hilgard has brought forward convincing proof that at least in the case investigated by him it might occur in addition to the unconditioned reflex, as a supernumerary movement, an entirely new response, which could not possibly be a substitute or a reproduction (4). Whenever, in fact, the hypothesis of sheer reproduction or reproductive combination has been used in the description of the highest psychological activities it has been found inadequate. On the one side, it is found not to account for the observed facts; on the other, it postulates a process on the existence of which careful examination tends to throw doubt. On the side of subjective psychology reproduction of the past is denied, as a working mechanism, by workers of such widely different interests as F. H. Bradley (5), F. C. Bartlett (6), Stout (6a) and the Gestalt group, of course in different contexts. And in the same way we find such objective psychologists as are best qualified to speak—as Lashley (7) and Hull (8), for example—rejecting the analysis of all behaviour into reproductive conditioned reflexes, and maintaining that the problem of the analysis of action is not yet solved. For much the same reason, then, that the classical association must be rejected as an explanation of the directed or orderly flow of thought as experienced, so must the conditioned reflex be rejected as an explanation of the orderly progress of the most complex action.

(4) *The Würzburg school*

It is well known that, at the beginning of the century, Watt (10) and Ach (11), of the Würzburg group, directed experimental attacks on the problem of the orderly flow of thinking, probably acting on a hint which Külpe dropped in 1893. They produced very similar solutions though they used different terminologies. Associationism was retained as the basic mechanism of thinking; but it was supplemented by another factor, which Watt calls the *Aufgabe* or task and Ach the 'determining tendency'. According to Ach, for example, the determining tendencies facilitated relevant reproductive tendencies and inhibited irrelevant ones. When, in a multiplication sum, I meet the figures 6 and 5, I do not put down 11, 1⅕, or 1, but 30. These false associations are ruled out, and the correct one facilitated, according to the theory, by the determining tendencies. The working of the determining tendencies answered the question which had apparently puzzled the Würzburg group, as to why, of all the associations clustering round a given presentation, to use their language, the *relevant* association should appear during the process of thinking. But their whole scheme was still based on the doctrine of reproduction. One becomes tired, in fact, when reading their papers, of the continued iteration of the phrase 'reproductive tendencies'. Particularly is this true of Ach's paper, who weaves a nexus of reproductive tendencies and determining tendencies reminiscent of the 'properties' attributed to objects by the mediaevalists. Because it is thus founded on the theory of reproduction the Würzburg solution is subject to the same criticism. This should be made explicit. The classical association theory claims that if two experiences occur together, and one of them later reoccurs, the other is reinstated. Watt and Ach claim that out of many candidates for reinstatement the relevant one is selected by means of some extra-associational factor. But it has been seen that the hypothesis of reinstatement is untenable. A further difficulty of the Würzburg theory will be mentioned later.

III. NON-ASSOCIATIVE THEORIES

(1) *Dynamic tendencies*

It might seem possible so to amend the doctrine of association as to replace the action of reproduction by one of representation. A given experience would then be followed by another in which a previously associated experience would be represented, though not reproduced. The *Aufgabe* or determining tendencies would render the process specific. Reproduction, such as seems to occur in the nonsense syllable experiments, would be a limiting case imposed by special conditions. This formula which is really the one proposed by John Stuart Mill might seem to provide a possible mechanism for the orderly process of thinking, and without contradicting experimental fact. Such a scheme, however, would not be so much an emendation as an abandonment of the associational hypothesis. For, as many workers have pointed out, including Prof. Warren in the passage quoted, as soon as the notion of reproduction is abandoned, we must assume positive forces at work making for change, active, non-associational principles by which the past is represented in the present. This alone gives an entirely different aspect to the whole picture. We now have the task of describing the nature and mode of action of such forces. Further, supposing the existence of such hypothetical forces had been demonstrated, we would not then be entitled to assume that the experiments of Watt and Ach, which were performed under the limiting case when they were negligible, could validly be used to draw conclusions concerning those processes of ordinary thinking where they were operative. In other words, to eliminate reproduction from the picture is not a negative, subtractive, change, in the hypothesis; it really involves the introduction of positive factors which would invalidate the experimental evidence. And apart from reproduction there are other serious difficulties.

Lewin's well-known experiments (9) have cast some doubt on the ability of one experience as such to cause another to arise because the two have occurred together in the past, whether the process is one of reproduction or of any other kind. The couplings or bonds between mental events are not, he claims, the motors of future mental events. That is to say, for Lewin the energizing force which causes one event in a train of thought to follow another is rather the total play of mental forces operative at the present moment, which uses past experience as purely inert material. Connexions between events are important but only as determining the form which mental events will take, not as initiating them. This point is misunderstood in Thorndike's criticism.

We are here brought up against one of the chief problems in the modern psychology of thinking. What is the relation of present to past experience in the mechanism by which one process follows another during thought? Does the past effect a dynamic intrusion on the present, in that bonds or connexions previously made tend of their own motive power, so to speak, to bring back past mental events in some possibly modified form? This is the conventional doctrine of association, or, in more homely terms, of habit. Or alternatively, is the past experience the inert, but nevertheless organized, material which is utilized in the process of thinking by the dynamic needs, stresses, wishes of the organism? This is the view of Lewin and originally at least of Freud. Or lastly, do the directional factors of thinking embrace a dichotomy, with both habit or bonds, and wishes or task, playing a dynamic part? This intermediate view is that of Ach and Watt, and presumably of Thorndike, though Watt is inconsistent on the point. It is not the purpose of this paper to make an exhaustive analysis of this problem, and in any case such analysis would be premature, for it is experiment alone that can decide the issue. It is, however, in place to point out that, rejecting pure associationism, each of the remaining alternatives has its own peculiar difficulties. If we adopt the Würzburg view that the mechanism of thinking has a double drive, that inherent in habit and that inherent in motive, then intricate problems arise as to the relation between the two. To elaborate an example already given: I wish to find the number of postage stamps in a sheet 7 stamps wide and 9 long. I say 7 times 9 are 63. Now in Thorndike's terms, there are many bonds between 7 and other numbers and words. There is 7 and 1 are 8, 7 and 2 are 9 and so on; also 7 days in the week, 7 ages of man, 7 pillars of wisdom, as well as 7 times 4 are 28, 7 times 5 are 35. Under any theory involving an associational background all these associations must be somehow involved when one says $7 \times 9 = 63$. We have seen that Ach's hypothesis of the determining tendency maintains that they are all inhibited, while the correct one is reinforced by the determining tendency. This may be so; but it does seem to be an extraordinarily wasteful method. It means that the major work involved even in such a routine process as the calculation of the number of postage stamps is spent not on the making of the one correct answer but on the inhibiting of the multitudinous incorrect ones. The emphasis seems to be in the wrong place. Somehow, it reminds one of the Irishman's definition of the way to make a cannon, by taking a hole and pouring some lead around it. The Würzburg doctrine has already been criticized because it depends on the external doctrine of reproduction. The present difficulty constitutes a further obstacle which the Würzburg doctrine must meet even though it can be so modified as to avoid the implication of reproduction.

On the other hand, to deny all dynamic force to habit as Lewin does seems equally difficult. Both experiment and common sense seem to point unequivocally to cases where sheer unmotivated habit persists simply because it is habit. One thinks for instance of the stereotypes, useless movements, in the animal mazes and of habitual mistakes in typing and speaking. All of the latter are surely not to be explained along early Freudian lines. That every thought and action is motivated by the immediate urges of the mind is indeed a proposition of Freud's which has aroused considerable opposition. Freud himself has later postulated a tendency towards reinstatement. It is conceivable that experiment may reconcile the paradox of the two opposing difficulties along Lewin's lines by establishing the proposition that the difference between motivated and unmotivated processes is one of degree only, motivation being a convenient term for a more inclusive integration. Even then, we should still have the problem of the relation of integration to that which is integrated.

It is worth noting with reference to the contribution of Ach and Watt that objective psychology has here again found itself travelling much the same path as subjective psychology a generation ago. Leeper's experiments on animals have caused him to distinguish between habit and the utilization of habit. As the result of training, his animals turned to the right or left at a T according as they were hungry or thirsty. "Motivation", he says, "can exercise a directive function in relation to habit utilization" ((12), p. 38). With appropriate changes of terminology, this sentence might have been written by Watt or Ach. Leeper is apparently unconscious of the relation between his experiments and the Würzburg work for he does not mention the latter. It is perhaps further worth noting that many maze workers have noted the fact of variability in performance from day to day. A day's run of the maze is very obviously not an exact reproduction of yesterday's. Certain investigators have indeed gone so far as to postulate a principle of spontaneous variation. This variation was easier to observe in the maze than in the conditioned reflex experiment, which is a limiting case, chosen to support the doctrine of behavioural reproduction; just as the corresponding limiting case, the nonsense syllable experiment, was used in support of the theory of the reproduction of experience.

The question of the direction of thinking may now be specified a little more closely. It becomes clear that we have in reality to do with two distinguishable groups of problems, or possibly with two aspects of the same problem. There is first a group of questions concerning what Lewin has called the motor of thought. What is the energizing principle by which successive thoughts or stages of thought arise? What causes the train of thought to move? There is secondly, a group of questions concerning the relevance of the processes when they do appear. What keeps mental processes relevant? Conceivably the energizing principle and the directive principle may be identical, but we are not entitled to assume this. For there might possibly be called into being a train of mental events which were irrelevant both as regards any external situation and as regards each other, that is, a train which was energized but not relevant. Though the results of experiment, rather than of common sense experience show that this is unlikely or at least very rare.

It may be seen that the association psychology derived both energy and relevance from the single principle of association or, in the later terminology, from the reproductive tendencies. Watt and Ach derived the energy mainly at least from reproductive tendencies, and explained their relevance by the *Aufgabe* and determining tendencies, though Watt is not entirely clear-cut on the point. Once again, the problem of mechanism encroaches on that of material; for while the question of energy is fairly clearly a question of mechanics, so to speak, the problem of relevance, as involving the nature of the processes that are brought into being, involves the question of material.

(2) *The theory of Selz*

The historical successor of the Würzburg school was Selz (13), who indeed numbered Külpe and Bühler among the subjects of his experiments. He was the first experimentalist to reject the conventional association explanation of thinking altogether. It is well known that for Selz thinking consists in the determined completion of a unitary *Wissen*, or knowing, or in the case of productive thought in the determined initiation of certain mental operations which he claims to be of a reflexoidal kind. The nature of what he calls the determination is obscure in Selz's account. What he does is to say in effect that the energy comes from the determination. But of the nature of such determination we seem to be told nothing, nor whence it comes, except that it is a factor in a unitary task, which is more inclusive than Watt's task. In another passage he identifies it with a modification of the will. In fact, Selz's solution of the problem why thoughts arise is the purely verbal one of saying that the task contains a determination. The non-explanatory nature of the statement is in large part concealed by the fact that he has borrowed the term determination from his predecessors, who had used it in an associational context. If the determination does not function by reinforcement and inhibition of reproductive tendencies, one is, however, justified in asking how does it function. It is really to the problem of the relevance of the processes that Selz has devoted his great acumen and industry, leaving that of the cause of their appearance unsolved and really unadmitted. Concerning his solution of the problem of relevance no statement will here be made except to say that there are grave difficulties in a proposed mechanism for thinking which uses the reflex of Sherrington for an analogy and gives the pupillary reflex and decapitated frog as illustrations.

(3) *The Gestalt theory*

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The other well-known non-associative scheme is, of course, that of the Gestalt school. For the members of this school a problem situation is one the perception of which sets up certain psycho-physical stresses in the organism. Very roughly speaking, thinking is the process by which these stresses are resolved. This process is, however, not one involving the present and its immediately engendered tensions alone; it works in conjunction with and through the trace-column, the masses of traces, ultimately of a chemical nature, left by past experience ((14), p. 207). During the process of release reorganizations of the total field occur which correspond in experience to what is known as insight. "Such movements as would increase the total tension in the field are excluded, and the dynamic situation demands such movements as will decrease the tension" ((14), p. 626). The stages by which thought proceeds are relevant because, the perceived problem being equivalent to a system under stress, such transformations occur as are specific to the resolution of the particular stress in question. The whole series converges towards the specific equilibrium demanded by this particular problem. The necessary energy is provided by the tendency of the system under stress to achieve a state of lower tension, like the mainspring of a watch. So that hypotheses are offered to account for both the relevance and the energizing force of the thinking.

It must be said that after reading the accounts given by earlier workers the Gestalt hypothesis is highly attractive. It has an organic quality not possessed by any of its predecessors, and one which seems to be more in keeping with one's actual experience and with modern ways of regarding the organism at work. But when it is examined, very grave difficulties appear. Of these I shall mention two only.

The hypothesis assumes that a certain quantum of energy resides in the original system under stress. When the stress is resolved, a lesser quantum is present, the difference being due to the energy consumed by work done. The argument thus implies a closed system, where the energy used in doing work plus the energy present after the work is done together equal that originally present. The physical postulate of the conservation of energy of course assumes a closed system. In fact, Lewin himself postulates "a greater or less degree of abscission" for his tensional systems. But clearly these systems-under-stress occur in the organism and are in fact partial systems of the organism. So that we have the notion of a system which is to be considered as a closed system, and which at the same time is a partial system of the organism from which it momentarily derives its energy. It is not impossible that the logic of such a state of affairs may ultimately be worked out. The body, in its wisdom, as Cannon would say, does preserve many constants in what may be regarded as relatively closed partial systems. Such, for example, are the constants involved in the composition of the blood. But we are far in psychology from the exact quantitative data and methods of the haematologists. It will be noticed that the psychological hypothesis under discussion is ultimately of a quantitative nature, in spite of Lewin's general disclaimer.

That the above is not merely an academic difficulty may be shown by the single illustration of errors. To make a mistake must use energy, just as much as to take a correct step. It should then be possible to discharge the system by making enough mistakes. Perhaps it is. But every mistake should diminish the original tension. This is contrary to experience; a mistake often seems to increase the tension. Koffka would say perhaps that there is here reinforcement from the ego system; but this would mean that the tensional system was not closed, as assumed. Once again, it is not suggested that these and other similar difficulties are insurmountable, but only that they have not yet been surmounted.

In view of what has already been said concerning the relation between the psychology of thinking and objective psychology, it may here be mentioned that both Selz and the Gestalt psychologists have used objective material in support of their hypotheses.

IV. CONCLUSION

This highly condensed survey of the explanations which have been given of the determination of thinking may seem to be very inconclusive. Pure associationism has been shown to have its difficulties; the Würzburg modification has others; the non-associational schemes of Selz and of the Gestalt school have equally been criticized. No satisfactory alternative has been suggested. So that the problems have, in a sense, been left hanging in the air. In extenuation it may be said that the history of the subject in the last fifty years has largely consisted in a rebellion against the associational theory, but a rebellion which has never quite succeeded.

And the account of an unsuccessful rebellion against an hypothesis felt to be unsound must necessarily have a certain inconclusive ring about it.

Nevertheless, it would not be true to say that nothing positive has been achieved. For the whole field has been progressively clarified and its details sharpened. When, for instance, we see the objectivists finding themselves involved in much the same difficulties and offering much the same solutions, in much the same sequence, as their predecessors a generation ago, we may rest assured that these problems and difficulties belong to psychology itself, not to the use of a faulty method. Further, at least two fundamental questions have been thrown into relief as a direct result of the work described. The one concerns the relation of past experience to the direction of thinking. The other is very closely related; it concerns the relation of motive to the processes energized by motives. Minor problems have also been clarified, hidden assumptions have been brought into the open. If the work of fifty years has not solved the major problem, it has at least defined it and shown where the difficulties lie. And that is perhaps more than half the battle. One who believes in our science may perhaps be pardoned for expressing the conviction that, although it is as yet unsolved, the riddle of the determination of thinking will ultimately yield to experimental attack; and that the successful attack will be made not by behaviourists nor yet by introspectionists but by—Psychologists.

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SOCIAL PSYCHOLOGY AND SOME PROBLEMS OF ARTISTIC CULTURE

By R. W. PICKFORD

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I. Psychological tendencies expressed in painting

It is particularly in relation to the problems of artistic, religious, scientific and the more highly developed forms of ceremonial culture that the inadequacy of many biological and psychological theories is felt most keenly. If we turn to painting as one aspect of artistic culture we find that painters often express aggressive,[1] destructive,[2] and rebellious[3] tendencies both in the subject-matter of their paintings and in their brushwork[4] and technique.[5] Many other tendencies are often expressed, but these are mentioned because they are sometimes overlooked. The work of art usually mirrors aspects of the artist's mind,[6] conscious and unconscious, as well as aspects of his cultural environment.[7] Where rebellious and destructive tendencies are not clearly apparent in a work of art, we often find their compensations—excessive perfection and integration.[8]

[1] E.g. Toulouse-Lautrec, *The Ringmaster*. Angelico, *Martyrdom of St Côme and St Damien*.

[2] E.g. Millet, *The Wood Sawyers*. Géricault, *The Raft of the 'Medusa'*.

[3] E.g. P. Breughel the Elder, *The Unfaithful Shepherd*. Delacroix, *The Barricade*.

[4] E.g. van Gogh, *Flowering Chestnut-trees*.

[5] E.g. Paul Nash, *Sunrise: Inverness Copse*.

[6] E.g. R. Mednikoff, *Head Waiter* (Read, H., *Surrealism*, Pl. 52). Paul Klee, *Mask of Fear* (Read, H., *Surrealism*, Pl. 43).

[7] E.g. Master of the Half Lengths, *Three Musical Ladies*. A. Rubliev, *Three Angels*.

[8] E.g. M. Laurencin, *Girl with Rose*. P. de Chavannes, *The Fisher Family*.

Even what is marked in a work of art by its absence may be of outstanding psychological[1] or aesthetic importance.[2] The balance of tendencies in the artist's mind is expressed in the balance and design of the painting,[3] and the person who appreciates the painting usually likes or dislikes it according to the degree to which it is appropriate to his own peculiar mental organization. The artist's conscious and unconscious mental outlook is based on his own motives and tendencies and reflects his social environment which depends on the biological tendencies of its group life. It is clear, therefore, that many psychological and biological ideas about social groups are closely related to artistic culture, in spite of their apparent inadequacy.

II. FUNDAMENTAL MOTIVATION TENDENCIES

Fundamental human motivations seem to be grouped round four chief modes of biological relationship between the organism and its social and physical environments: (1) food seeking and alimentary; (2) defensive and aggressive; (3) sexual and reproductive; (4) comradely and social in so far as not implied by the other three. The exact number of particular tendencies may be difficult or impossible to determine. These modes cannot be treated as stereotyped reaction patterns or as mechanically separate classes of response. They seem even to defy logical analysis, because the relationships between them, at least in man, must be worked out more in terms of emotional meanings than of objective reaction patterns. The meaning of food seeking, for instance, may from the very start be bound up with sexual and comradely meanings, and with the desire for protection, and may easily come to involve defence or aggression. In the same way, the fundamental social tendencies of assertion, comradeship and submission are often linked up in an intimate way with sexual, defensive, aggressive or feeding tendencies. At the same time, it does not seem to me possible to say that human motivation is all attributable to an originally undifferentiated *libido*. We have to consider what modes of relationship with the social and physical environments are chiefly represented in a given human response or interest, and how that particular expression of them has arisen in the life of the individual concerned. These problems can, if we insist, be treated in terms of the individual alone, but in fact they always imply social psychology, because the cultural and social patterns must be taken into account as conditions of response. Human behaviour and motivation are tremendously influenced by the cultural pattern and social environment, and, although the fundamental motives may be instinctive, actual behaviour can never be explained solely by reference to specific instincts.

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[1] As pointed out recently by Dr K. M. Abenheimer. Cf. the anonymous painting 'Abstraction' (16).

[2] Cf. Schubert's Unfinished Symphony.

[3] E.g. Matisse, *Woman before an Aquarium* (showing conflict). Velasquez, *Venus and Cupid* (showing harmony).

It is necessary to point out that these ideas must be treated as dissecting instruments for the investigation of behaviour and experience, and must not be allowed to become a scheme of faculties which are supposed to explain the living organism. It is always the group or the organism which 'tends', just as it is the organism which perceives, feels, remembers, or imagines, and it always does so in relation to particular conditions or circumstances. The objections which are now frequently raised against the instinct theory are sound because that theory has been misused. They are properly directed against the misuses and not against the theory, for psychologists have been inclined to forget the organism in talking about the instincts.

It is interesting that the psychological principles underlying aspects of advanced cultures, such as painting, are not widely different from those underlying primitive beliefs and practices. This, although many people will say it was obvious from the start, is important because it is by no means generally accepted. Many people, after admitting the excellent qualities of primitive art, habitually assume that their own great artists are in possession of realities altogether inaccessible to primitive man. Indeed, it is well known that logic is able to prove whatever the logician believes to be true. It would not be surprising to find that other branches of material culture and learning developed and expanded by psychological steps not remote from those of art.

III. TENDENCIES EXPRESSED IN PAINTING AND THEIR RELATIONSHIP TO PARTICULAR MATERIAL

There are two very interesting questions: How far is painting the expression of unique and how far of general tendencies; and how far do those tendencies express themselves by using material peculiar to them? A moment's reflexion will show that the number of tendencies concerned in painting, and their variety, are very great. Many of them, such as vanity, may be expressed directly. Others, such as the desire to make money, are involved indirectly. There is, however, no simple fixed division between the tendencies which are directly and indirectly expressed, and in practice the complications are very great indeed. If, however, we whittle away one by one the tendencies which, though often involved indirectly, are not of direct importance either to the attitude of the artist when painting or of the person who enjoys pictures, it may be possible to reach a minimum which is absolutely essential. Such a minimum would be the indispensable nucleus of artistic expression and valuation, and the problems of its existence and how it may be defined, are very difficult. Such problems cannot be dealt with here, but it would appear that the absolute essentials in artistic expression and appreciation are better described in terms of the harmonious working together of other tendencies than as unique aesthetic tendencies themselves (cf. (1)).

So far as the evidence can indicate, a vast number of aesthetically unessential tendencies are normally involved in painting, but they are psychologically essential to the particular form which painting takes in given circumstances. The Russian icon may have taken its particular form because of the special combination of tendencies in its religious setting, but these religious tendencies may be aesthetically unimportant. In this article, which is not a study of aesthetic values, no evidence has been collected about the essentials of aesthetic attitudes. Painting appears to be an everyday interest, and those who think art does not contribute to our everyday well-being are probably in the wrong. It does contribute to ordinary life, and is normally present in every culture which is not, from some cause, specially narrow.

The question of the special material, if any, dealt with in painting, requires a similar treatment. If an essential 'material' can be found, it will probably be best described in terms of harmony of visual rhythm and design. Visual art, however, may express its designs in subject-matter related to any tendencies normal and abnormal, and therefore may be relevant to any aspects of ordinary life.

The problem of the relationship between the tendencies expressed in painting and the material they deal with is of great interest, but again, exceedingly difficult. The full complexity of this problem is brought out when we consider, for example, the Russian icon (2), Rajput painting (3) and Bushman art (4). All of these, without doubt, are largely of religious import. None of them is intelligible to us in its original meaning any more than West Christian iconography can be to a normal Chinaman. It is only by a laborious intellectual reconstruction that the relationships between Bushman drawings and the tendencies which were expressed in their making can be seen even dimly. Indeed, for Bushman and Rajput painting and the Russian icon, we are hardly in possession of the data necessary to make this reconstruction at all. Even where such material is more available, as in the pre-Raphaelite movement, much difficulty is found, and attempts to take, in imagination, the place of the artists are exceedingly precarious.

In spite of this, however, the appreciative observer is able to see the genius in all these forms of art, and Bushman paintings are comparable in fascination with Russian icons. It is tempting, therefore, to conclude that the most central core of tendencies in painting does have a close relationship to the material—harmonious and rhythmical designs—in which it is expressed, but that the less relevant tendencies, connected with religion, warfare and everyday life, are related to their subject-matter mainly by the accidents of circumstance. It is accidental that the Russian icon deals with Saints and the Virgin Mary, and that Bushman painting deals with rainmaking and the springbok. These aesthetic accidents, however, are circumstances of the greatest interest for the history of culture. Further discussion of the existence of essentially aesthetic tendencies or material, and of the relation between them, will unfortunately have to be avoided.

IV. INFLUENCE OF THE SUPER-EGO

In psycho-analysis the 'super-ego' is conceived as a complex of tendencies largely unconscious, arising from the individual's social contact with his parents and other adults, since infancy, and expresses, in the main, his capacity to control himself in such a way as to fulfil what he feels to be their demands. Often there is much conflict between the demands of the super-ego system and the demands of 'reality' and those of the persons's own instinctual life; or the relationship may be ambivalent —partly made up of conflicts and partly of acceptances. Any persistent tradition handed on from master to pupil or followers might be explained by saying that the master had entered into the super-egos of his followers and thus determined the direction of their interests. Any obstinate rejection of cultural influences or traditions which have become specially identified with the names of great masters, conversely, may be explained as due to reaction against the control of the hated super-ego. It might be said, that in Cézanne's (5) artistic development these two influences are present. Sharp antagonism to his father and to domestic control and discipline of any kind was transferred to social restraint and to cultural conventions: he became a violent artistic rebel. On the other hand, the relationship with parental control was not without its positive aspect, and, where not presented in direct form, cultural tradition had a strong attraction for Cézanne. He copied paintings in the Louvre and chose his own masters—Daumier, Chardin, Poussin and others. Thus the rebel fell into what was actually a very strong tradition, and became its greatest living exponent, though long unrecognized.

It is necessary only to mention Rossetti's enthusiam for Dante and early Italian literature and painting, combined with his rejection of his father's direct influence; Corot's objection to becoming a draper like his father, together with his dependence on the very tradition that later influenced Cézanne; the extremely close connexion between Russian icon painting and the social and cultural authority of the Eastern Church; and the collapse of Mogul miniature (6) painting when the personal influence of the Indo-Persian emperors was withdrawn, to remind the reader that there are innumerable events in the history of painting which might be explained by the super-ego theory.

It may be said that Mogul painting was due to a combination of Indian and Persian traditions under leaders who entered into the super-egos of their followers, and that the Persian tradition itself was carried forward to that point by a series of regal ancestors of these leaders, from the time when the Mongols conquered China in the thirteenth century. These ancestors were all patriarchs of nomad hordes, who might awaken admiration, envy and resistance in some of their followers, in accordance with the super-ego theory. Feeling inferior and barbaric in China, these ancestors had borrowed her culture for prestige, and carried it to other parts of Asia. China, however, is the great land of ancestor worship, and it would be easy to argue that the very essence of Confucianism is the combination of fear and adoration of the dead, and that every dead man worshipped is a symbolic super-ego, who watches closely over the living to see that everything is done in exact accordance with the conventions of many hundred years standing. The Mongols might be said to have borrowed unconsciously the super-egos of the Chinese and to have carried them to India. This theory may account for persistence of tradition, but a weakness is clear when the discovery is made that the outstanding period of artistic and other constructiveness in China was part of the T'ang Dynasty (A.D. 618–905) when religious freedom existed. Waley[1] points out that the three centuries of this dynasty were characterized in turn by Buddhist speculation, by lyric poetry, and by the Confucian revival. In the eighth century A.D. Manichaean Christianity existed side by side with Buddhism and Zoroastrianism, and this part of the T'ang Dynasty was China's greatest period of outside contacts and expansion. In A.D. 845

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[1] Cf. (7), Chaps. VIII–XII; also Giles (8).

the conservatives triumphed, and Christians, Zoroastrians and Buddhists were suppressed, with consequent destruction of paintings. The growing interest in romantic landscape painting after that time may be traceable to desire to escape the rigours of Confucianism. Only after the Confucian revival set in did the nation become isolated and gripped by convention, and ever since cultural construction has been steadily on the decline. The super-ego theory has some importance, especially in connexion with conservation and cultural decline, but, clearly, other principles will have to be used to account for constructiveness.

V. REPRESSION AND SUBLIMATION

A complex system of processes, conveniently called sublimation, seems to be very important in painting. It is widely recognized in psychology that tendencies, usually in the form of wishes or desires, which conflict with other tendencies in the same person, or with social influences expressed in general social pressure or in the will of other persons, may be subject to unwitting repression. Such repression is one way in which the organism naturally protects itself against overt conflicts, sometimes with disastrous results. Repressed tendencies usually persist unconsciously and may express themselves in disguised form as dreams and symptoms of various kinds. Feelings and emotions may pass into the unconscious in the same way, and reappear in disguised form also. The degree of repression and the nature and extent of the disguises may be very varied indeed.

Much of the content of pictures is disguised material which has been subject to some degree of repression, and many of the special interests of artists in painting may be due to repressed tendencies. If this process of indirect and sometimes refined expression of crude but disguised tendencies and material is called sublimation, then sublimation is important in the psychology of painting. It is interesting to observe that the ultra-modern art of the Surrealist group, judging from the Surrealist Exhibition in London, 1936, differs from normal art rather in degree of disguise than in the actual content of sublimations. In a conventional nineteenth-century painting a bull, a cow and calves may be shown, standing near a pond under some trees. The animals have a distinct hint of the human about their faces, and the ensemble is carefully arranged so that the sexual organs of the bull are not to be seen. A Surrealist painting might show a life-size human arm with fist clenched, not draped but thrust up to the elbow through a wooden quoit ring painted scarlet. Whatever philosophical or aesthetic meaning the painter may find in his work, from the point of view of the sublimation theory the essential difference between these two pictures is that the symbolic transformations of the sexual *motif* in the bull and cow painting are far more complete than in the other, which is all but a plain rendering of the penis inserted into the vulva. Each subject, however, gives the painter great opportunities for fascinating use of design, rhythm and colour, and that may be its true, artistic meaning.[1] As already pointed out, it would be rash to assume that the sublimations and symbolisms in particular paintings could be readily and fully determined. It is clear to us that Rossetti's interest in painting his sister, his wife and Mrs Morris may have been relatively simple sublimations of sexual desires directed towards them. Rossetti may or may not have been aware of this. Most painters, however, who are not altogether stupid, must realize that every drawing of the nude is a sublimation. On the other hand, the full nature of the sublimations in Daubigny's paintings of the Oise, to the scenery of which residence with his nurse at Valmondois had introduced him, will probably remain as big a mystery to us as it would have been to him had he been asked to explain it.

Clearly, sublimation itself is the indirect expression of social influences, or of the conflicts they engender in the individual, and to explain the part it plays in artistic constructiveness requires also the explanation of those influences. Though every painting of the nude may be a sexual sublimation, the human figure has a value because it gives unique opportunity for the construction of rhythmic and harmonious designs. The important points are that sublimation itself does not explain cultural constructiveness, and that to discover a painting to be a sublimation of certain tendencies does not fully explain its artistic interest.

In this study it is tempting to cite instances, such as that of Goya, who is said to have had twenty children in wedlock, which seem to disturb Unwin's (11) theory that cultural constructiveness is directly the product of sexual restriction. While little support may be found for this theory, however, it would be unwise to lay very much stress on such instances. Goya's reproductive and his pictorial fecundity may have been a dual expression of the same repressed desires seeking their outlet. Great sexual energy is often an expression of passionate struggle against repressions rather than the product of straightforward desire. Unwin's theory must be investigated by a detailed study of special case histories.

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[1] This limitation would be denied by the surrealists themselves. "Completely devoted to the imagination and its charms, surrealist poetry aims at exteriorising all man's desires, all his obsessions and his despairs: it gives him the means to free himself, to venture forth." "But I have only shown one of the aspects of Surrealism. There is, in addition to its poesy, its social function, its political function, its complete adherence to the principles of dialectical materialism, its revolutionary position, and its struggle against patriotism and the bourgeoisie." See Read, H. (9) pp. 218 and 246; also (10) Ch. v, where there is a discussion of the Freudian theory of the artist.

VI. THE GESTALT THEORY

It is not necessary that the Gestalt (12) theory should be discussed at length, because most readers will be quite familiar with the general meaning of this theory. In social psychology it depends chiefly on stressing the configurational aspects and the patterning of social groups. In the history of art, for instance, the origin and development of either the Barbizon or the pre-Raphaelite groups might be expressed in Gestalt terms. Obviously, however, the question of absolute origins must be put on one side, as leading back to infinity in social history, and every individual must be treated as approaching the particular social developments being studied, with his own frame of ideas and his own tendencies already socially determined. In either the pre-Raphaelite or Barbizon groups certain individuals, with such predetermined attitudes, ideas and tendencies, are found to meet through circumstances more or less unconnected with their particular artistic aims, and very quickly make personal contacts, as a result of which more permanent attachments, and then a special group, are formed. The development of these attachments can be well described in terms of Koffka's principles of the similarities of moods and emotions among persons who may or will form a group, and of the 'incompleteness' of the isolated individual. The main characteristics of the groups formed can also be expressed in his terms. Each group rapidly becomes a unified system, segregated from its social background, stable and articulated within itself. The degrees of unification, stability, segregation and internal articulation differ in any two groups, but these characteristics are always present in some degree. Koffka's treatment of the products of social activity, the mass of material and ceremonial culture together with traditions and social habits, is also easily applied to such groups as the Barbizon and pre-Raphaelite painters. Each developed in a special social setting, selected and rejected aspects of that setting, and constructed a new phase of culture upon the framework of what was selected.

In all these ways, the Gestalt theory is not remote from the theory of individual and social tendencies. Apart from purely verbal differences, the two views are closely comparable. There remains, however, the difficulty of Koffka's claim that psychology must seek the explanations of its data outside its own realm; in short, that it must find those explanations in physics. This is an *a priori* claim, which remains to be tested like any working hypothesis, and is opposed to the commonly held view that, in the study of behaviour and experience, mental explanations are required. It is not the aim of this paper to weigh the evidence in favour of each view, but the writer thinks that, while the problem is not likely to be settled during the present century at least, the facts of organic life appear to be against Koffka's claim (but cf. Waddington (13)). The constructive nature of many organic and social changes is far from new as a stumbling block for theories of the organism based on physics. Koffka's treatment of the relation between psychology and physics is original, and makes the unification of these sciences appear more likely than it has appeared before. The principle that these sciences ought to be unified, however, is itself a working hypothesis, which has no claim on us except in so far as it may give the most complete and economical explanation.

VII. Biological tendencies

The relations of the organism to its environment are of fundamental importance for biology, and a great number of biological problems arise from their study. The environment, of course, may include other organisms as well as purely geographical conditions. The organism, viewed biologically, is a unit which strives to survive under given conditions, and fails or succeeds to some extent. The peculiar structure and behaviour of any organism have to be understood in terms of this effort to survive and the nature of the environment. This proposition is not fundamentally altered by mechanistic biologists, because they study the same problems, but reject all explanations which are not mechanistic. In the study of groups we are concerned with biological questions. The social group, again, is a unit which maintains itself with more or less success, under given conditions which include other groups as well as geographical circumstances. As MacCurdy (14) points out, the group itself 'strives' towards survival, in a certain sense; and what happens to a given group is to be understood in terms of its striving, its structure and the external conditions for and against it. A group usually has a certain period of activity and then fails to maintain itself. This is also what happens to all collections of organisms which are sufficiently integrated to have one or more characteristic social tendencies, even if not integrated enough to appear as sharply marked groups.

Since it is concerned with the effort of the social group after survival and with the conditions under which this effort occurs, the biology of groups is difficult in a particular way to apply to the history of painting. Mankind would appear to be partly controlled by tendencies other than those immediately related to survival value, else activities so remote as painting from self-protection, reproduction and feeding would not be included in the everyday life of human groups.

It is worth considering just how deep is the separation between painting and these biological tendencies. Many painters have successfully earned their livings by their art; there is no simple connexion between ability to paint and sexual impotence; and most painters are quite as capable of defending themselves as are other ordinary members of society. There is no evidence that the best painters are less able to earn money, less sexually active or less able to look after themselves than bad painters. Indeed, though it is often called a 'fine' art, painting is comparable with any good trade. There is a less deep gulf between it and food seeking, reproduction or self-protection than many people think, for it is able to combine effectively with any of them. To this warning for people who might be inclined to overstress the idea of pure art, however, it must be added that among the interests of mankind there do seem to be some which are not strictly biological, and the interest in art is among them. It is clear that biology cannot fully explain human life, just as physics cannot fully explain biology. Nevertheless, the biological aspects of human life are of great interest.

VIII. THE INDIVIDUAL AND THE GROUP

If we turn to the position of the individual, it is futile to regard social life as created accidentally by the coming together of individuals. Social life is not even something simply thrust upon the individual by circumstances unrelated to his active tendencies. On the contrary, he is incapable of being happy without it, and normally adopts a constructive attitude towards it. The relationship of the individual and the social group is always biologically complementary in essence, and only when failure and infantile and other conflicts have set up acute discords, is the attitude of the one to the other destructive. In fact, biologically, social life never has a beginning for the individual at all. It arises together with the bodily relationships and organic continuity of parents and offspring. Solitary young do occur in the animal kingdom, and young which are normally neglected by the parents but which form groups on their own are frequently found (15), but neither condition occurs in mankind or any of man's near relatives.

The development of social life, considered from the point of view of the individual, is a process of differentiation, in which the simple possibilities of infancy become elaborate and complex. In this elaboration the attitudes of the individual become more specific, and he selects parts and rejects other parts of the customs and cultural material of his own and other social groups. He is then the focus of constructive changes going on in his group. During this development he may meet with frustrations so great that he loses touch with surrounding changes, but since recluses like Cézanne may be highly constructive, there is no simple relationship between conviviality or obvious public helpfulness and social construction. From the point of view of society the individual is a special focus of activity and does not require to be driven into a useful path, but, if forced to do what will normally be his own desire, he may become destructive and rebellious. What is true, in this way, of the gifted genius, is true in due measure of the ordinary person.

Obviously, a very important biological function of small groups, which, like the pre-Raphaelites and the Barbizon painters, exist within a wide social setting, is that they support the individual during the period of his most difficult struggles. This kind of group is a stepping stone to wider social activities. While the members need the support of immediate friendly relations, they stay together, but when this need has been fulfilled and is past, or when their interests have developed and changed, they find some occasion, probably trivial and such as might have been passed unnoticed before, and quarrel.

Groups, however, must not be viewed only from the individual's point of view. They are also organic units which pursue a certain course of social constructiveness and rebellion, and are formed at times of particular coincidences only one set of which are the desires and needs of individual members. There is much to support the view that, if those particular persons had been absent or different, a group might have been formed on broadly similar lines with another personnel. This would be possible only in the presence of able men, of course; it is well known that the loss of a great leader is often fatal to a group movement. However, it is clear that the group itself moves towards social change and construction, and, once formed, carries the members along.

After a period of activity the group or movement may sink before circumstances too adverse to be withstood, or before new tendencies which it cannot assimilate. In this the group fails to secure its own survival and fails to be a source of inspiration to new members. One adverse circumstance may be that the material dealt with has been at least temporarily exhausted by known methods. Apart from this relatively infrequent possibility, the adverse conditions may be of many kinds. When a group or movement sinks before adversity, it is well known that self-conscious efforts of individual members are not successful in resisting the decline. The individual is powerless where the group fails. Nevertheless, many of the methods, standards and actual products of a movement or group may remain in the hands of successors, and be vital to their progress. From the biological point of view there is no reason why the tendencies of a particular group might not run on to excess in a given direction, and destruction might come through a sheer overloading of influences and material originally helpful and adaptive. The only essential condition of this is that there should be no destructive adversity. A vast number of tendencies are preserved in our own society merely because there is nothing strong enough to destroy them. In all these and many other ways the painting group is based on a biological footing.

IX. CONCLUSION

In conclusion, it seems that the problems of artistic culture must be considered in the light of psychological knowledge of the individual and the social group. The individual's chief tendencies in relation to his social and physical environments are food seeking; sexual and reproductive; defensive and aggressive; and comradely. These are not mechanically separate 'drives', but are tendencies emotionally interrelated in complex ways peculiar to each individual himself. Painting expresses many and varied interests, but there seems to be no specific artistic tendency by itself. If there is a specific aesthetic material dealt with in art, it seems to be the rhythm of pattern, design and colouring, but even this is always intimately expressive of the artist's personal life in symbolic ways (16). Strictly speaking, 'abstract' art is non-existent, and all art is really full of meanings related to everyday life, expressed with varied degrees of clarity.

The Freudian process of sublimation is important for the psychology of paintings, for every picture is a sublimation, usually of many tendencies organized together. The super-ego is also important, and much of the influence of persistent traditions may be explained by it, though social constructiveness seems to require other explanations. The social group of artists, which is often an important aspect of artistic culture, is like a biological system which takes part in the struggle for existence, and which supports its members through periods of social and personal difficulty. In turn it expresses the main tendencies of the wider cultural environment of its time.

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FATIGUE IN TERMS OF THE ELECTRODERMAL RESPONSE

By CHRISTIAN A. RUCKMICK

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I. Fatigue as a topic for experiment

From the study of symptoms of mental fatigue made by the psychiatrist Sikorski (1), in 1879, to the carefully controlled experiments of Lapicque, is a matter of only half a century. During this period hundreds of books and articles have appeared considering the subject from many angles, and the list of contributors would be equivalent to a roll-call of the most prominent psychologists and physiologists of the world. As techniques and physiological knowledge have developed, so have the theories, which have been advanced to explain the varied facts, taken on a great many different forms. When in 1891 Mosso picked up quail which in their exhaustion had flown blindly against the tree trunks and the white walls of houses along the Italian beach on their northward flight from Africa, he sought to establish the fact that in "extreme fatigue cerebral anaemia is produced and that this anaemia may diminish the power of vision" ((2), p. 3). Lapicque suggests to-day that fatigue is analogous to curarization. Between these extremes lies a long winding road of progress in technical knowledge based upon the perfection of the scientific tools of research. Already Lapicque's interpretation has been questioned by Lindsley and others, but such appraisals surely spell further improvement in the understanding of the complicated concept of fatigue. How complex in one instance alone the problem of fatigue is has been clearly pointed out by Offner who stated as early as 1910 that "these and similar phenomena are psychic symptoms of physical fatigue, and show us that bodily activity results in mental, as well as in bodily fatigue" ((3), pp. 9f.). Later on he reverses the situation by calling attention to the physiological results of mental activity: "Moreover, the unconscious, or, as they are termed, purely physiological processes, are affected by fatigue through mental work" ((3), pp. 15f.).

Our problem is obviously, then, a psychophysiological one. In that connexion it is therefore not amiss to apply a technique that is rapidly gaining prominence as a form of electrical manifestation of bodily condition and activity, namely the electrodermal response (E.D.R.). Since the sweat glands are tied up chiefly with the widely ramified autonomic nervous system, it should be illuminating to discover how this mechanism reflects mental and physical fatigue.

Further, and more directly in line with other research problems on feeling and emotion in the laboratory, is the need to envisage the manifold factors which are reflected in the electrodermal response. Until we can identify the phenomena which influence the activity of the sweat glands, we cannot say that the responses which we obtain in affective or emotional situations are truly indicative of feeling, or believe that we are holding all other factors constant in an affective experiment. For a series of published researches of mine have shown that we must approach the electrodermal response with due caution. Affective phenomena are frequently indicated but so are many other mental and bodily processes. Movements of certain types, for example, as shown by Schaefer's work (4), are characteristically accompanied by periods of recovery about twice as rapid as those for emotional experiences.

Movements are not the only difficult items with which we have to deal in the affective area. We must study analytically one after another of the contaminating factors in this response. As Davis has noted: "With such wide variability in the reflex the most necessary contribution to knowledge of the reflex seems to be the discovery of more participating factors which can be controlled experimentally. With an increased knowledge of these factors variability would be lessened and further research rendered less laborious" ((5), p. 59). He stated that most attention had been given to "the discovery of bodily processes concurrent with the reaction" and almost no attention had been directed to "research on the mechanisms of conditioning, adaptation, facilitation, fatigue, etc., by use of the galvanic reaction" ((5), p. 8). He therefore proceeded to study several of the conditions accounting for individual variability, especially that of facilitation. For some time we have been impressed in our work with the effect of such general mental and bodily states in our observers as euphoria, relaxation, attention, tension, and fatigue on the recorded deflexions. Bagchi & Greenwald (unpublished work) have shown that electrodermal changes which are usually associated with involuntary activity can be definitely modified by different types of attention, or mental set, and that these types of attention or mental set can be roughly identified by the varying degrees of conductivity resulting from them. Strained attention is accompanied by a greater degree of variability in dermal conductance than quiet attention.

II. EARLY EXPERIMENTS

Our first direct, although preliminary, attack on one of the persistent problems involved in many, if not most, investigations in the affective area, namely, that of mental and bodily fatigue, was published seven years ago (6). At that time we did not fully realize that the Hathaway apparatus would not give us dependable quantifiable results. The galvanometric readings, relatively crude as they were in the earlier days, did indicate, however, that there was a general tendency toward a consistent decrease of bodily resistance during the state of fatigue (Tables I, II). In many of the simultaneous readings taken with the Hathaway apparatus

Table I. *Average galvanometer readings* (*Wheatstone bridge settings*)

Observer	Normal	Fatigued	Type of activity
P	38000	43000	Mental and physical work
P	33500	11690	Mental and physical work
P	8800	8700	Mental and physical work
Jk	6290	30390	Long walk
Jk	32790	88990	Mental and physical work
Th	4500	15090	Long walk
Wb	88000	64000	Mental and physical work

Table II. *Fatigue series*

	Galvanometer readings			Systolic blood pressure		
Observer	Normal	Fatigued	Increase or decrease	Normal	Fatigued	Increase or decrease
R	102	180	+	118	112	−
O	60	55	−	106	116	+
R	91	54	−	114	120	+
R	9	6	−	112	118	+
R	142	161	+	112	122	+
O	46	43	−	120	112	−
R	84	46	−	118	120	+
R	75	68	−	116	104	−
O	55	65	+	106	102	−
O	95	77	−	106	103	−
W	68	74	+	106	110	+
W	73	60	−	85	105	+
W	80	60	−	106	94	−
R	32	24	−	112	126	+
R	112	122	+	120	120	
			5+ 10−			8+ 6−

we obtained contradictory results, i.e. results which showed an increase in bodily resistance and were therefore opposite in sign to those obtained with the D'Arsonval galvanometer (Table III).

Table III. *Normal* v. *fatigue conditions*

	Galvanometer				Hathaway			
Observer	Normal	Fatigued	Difference	Increase or decrease	Normal	Fatigued	Difference	Increase or decrease
A	3500	2600	900	–	37	50	13	+
A	2600	900	1700	–	60	37	23	–
A	1600	900	700	–	32	39	7	+
B	4500	2600	1900	–	39	12·5	26·5	–
C	3300	2500	800	–	30	40	10	+
C	2500	1300	1200	–	43	38	5	–
D	2500	1500	1000	–	51	42	9	–
D	4000	3300	700	–	30	33	3	+
D	2200	1300	900	–	35	44	9	+
D	4100	2900	1200	–	40	47	7	+
D	1420	990	430	–				
D	1500	1300	200	–				
D	.	.	.	.	88	112	24	+
D	.	.	.	.	94	138	44	+
E	1150	1130	20	–				
F	2949	2620	329	–	143	160	17	+
F	2070	1550	520	–	31·7	36	4·3	+
F	3290	2230	1060	–	30	28	2	–
F	3100	2100	1000	–	41	47	6	+
F	4300	2600	1700	–	40	29·5	10·5	–
F	2510	2300	210	–				
F	9500	8100	1400	–				
F	3050	2610	440	–				
F	.	.	.	.	95	104	9	+
F	.	.	.	.	90	115	25	+
F	.	.	.	.	62·5	40	22·5	–
F	.	.	.	.	46	50	4	+
F	.	.	.	.	55	78	23	+
F	.	.	.	.	30	32	2	+
F	.	.	.	.	39	51	12	+
F	.	.	.	.	24·5	27	2·5	+
G	4200	2700	1500	–	36	37	1	+
G	4600	2400	2200	–	36	34	2	–
G	2800	2000	800	–	35	33	2	–
G	3500	2700	800	–				

Galvanometer readings in ohms of resistance.
Hathaway apparatus readings in potentiometer units.

These experiments were continued in great detail with the Hathaway apparatus and gave us a definitely decreased potential or increased resistance during the state of fatigue in the twenty-nine trials which are described in my recent book (Table IV) ((7), p. 266). Although I questioned the results at the time on the basis of the oft-repeated statements that the Hathaway apparatus could not be depended upon for any quantitative comparisons, it seemed to me that at least the direction of

Table IV. *Mental and physical fatigue*

Trial	Normal	Fatigued	Increase or decrease	Trial	Normal	Fatigued	Increase or decrease
1	62	67	5	16	63	57	-6
2	57	51	-6	17	75	71	-4
3	72	73	1	18	52	53	1
4	55	49	-6	19	51	49	-2
5	59	54	-5	20	58	53	-5
6	48	44	-4	21	63	60	-3
7	78	79	1	22	71	70	-1
8	73	68	-5	23	58	55	-3
9	57	55	-2	24	74	70	-4
10	69	63	-6	25	72	68	-4
11	47	49	2	26	55	51	-4
12	59	53	-6	27	51	49	-2
13	64	60	-4	28	63	60	-3
14	73	69	-4	29	79	73	-6
15	51	49	-2				

All readings are potentiometer readings on the Hathaway apparatus.

change was sufficiently large to justify the statement which I made in my treatise, namely, that fatigue operated in a direction opposite to that of feeling as shown in my Figs. 1 and 2 ((7), p. 367) (see p. 216). In a critical and well-controlled series of trials Forbes showed that the size of proportional neither to actual change in resistance nor to the percentage the deflexion was of change in resistance in the subject's circuit (8). These data, furthermore, corroborated the results reported by Strauss (9), who stated that the rate of recovery indicated the degree of fatigue obtained during the experiment. Seward & Seward (10) showed, however, that a continued negative acceleration in the decline of resistance during an experimental period of 8 min. was symptomatic of the degree of fatigue produced.

III. A REVISION OF METHOD

In order to check my own previous results and the above-cited data I attacked the problem again, using great care to keep as many mental, physiological, and physical factors as possible under control. Analytical introspective records were obtained, the time of day when readings were taken was kept constant, the temperature and humidity were accurately recorded, the shielded sound-proof room was used for all experimental trials, the instructions were carefully followed, and the series of items which were presented to the subject were rigorously carried out. A set of twenty problems in mental multiplication, of the order of 54 × 17, were assigned to the subject to compute, while painful and unpleasant stimuli were being presented as distractions. These stimuli included penetrating and extremely vile odours like pyridin, carbon disulphide, ammonium sulphide, asafoetida, and carbon tetrachloride, annoying electric shocks on the tongue, pin pricking, scolding, nagging, and the like, distasteful solutions on the tongue, like acetic acid, and in dilution, citric acid, and muriate of quinine. There were also vigorous exercises, such as lifting the body horizontally from the table on hands and toes up to 20 times, etc. The introspective records gave interesting qualitative pictures of extreme bodily fatigue and mental lassitude. Photographs of facial expression further revealed the appearance of a sort of "all-in" attitude. Almost all of the experiments were done on myself as subject because of the somewhat exacting conditions of the experiment. Oddly enough the state of exhaustion which was more complete than that following any mental or physical toil which I have ever "sweated through", did not show any mental effects beyond about an hour after the experimental series.

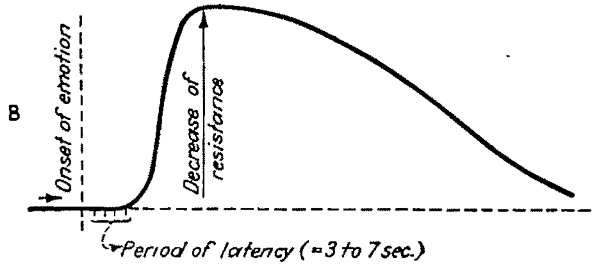

Fig. 1. A typical affective deflection. Reproduced from *The Psychology of Feeling and Emotion*, by Christian A. Ruckmick, through the courtesy of McGraw-Hill Book Company, Inc.

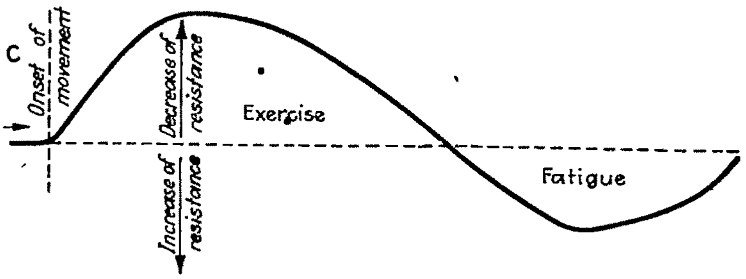

Fig. 2. Curve of fatigue as related to curve of movement. Reproduced from *The Psychology of Feeling and Emotion*, by Christian A. Ruckmick, through the courtesy of McGraw-Hill Book Company, Inc.

The results were surprising. The latest improved model of the Greenwald galvanometric apparatus was used[1] and photographic records were made. It will be seen from Table V that almost all the final readings showed decreased resistance as compared with the normal or originally balanced readings. These figures check, therefore, with our earlier galvanometric readings but are diametrically opposed to the readings of the Hathaway apparatus which I am now willing to discard but which I cannot easily explain. It is evident from these fairly consistent results that we must regard the phenomena of fatigue, when they set in, as of the same direction as that which characterizes both affective and motor responses, namely, a *decrease* in resistance or an increase in counter-potentials from the sweat glands. As far as we now know, then, it would seem likely that fatigue symptoms of both the mental and the physical sort would be accompanied by a retarded return of the deflexion to the base-line or, in other words, a delay in the time of recovery.

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[1] Derived from that described by Greenwald (11); but the present model more closely resembles the circuit described in (12).

Table V. *Fatigue series with Greenwald apparatus*

	Normal		Fatigued		Difference	
Date	E.D.R.	S.B.P.	E.D.R.	S.B.P.	E.D.R.	S.B.P.
19. ix. 36	63,000	154	48,000	160	−15,000	6
24. ix. 36	55,500	158	73,500	140	18,000	−18
29. ix. 36	70,500	152	37,500	162	−33,000	10
1. x. 36	67,500	158	82,500	162	15,000	4
9. x. 36	85,500	160	81,000	168	− 4,500	8
15. x. 36	82,500	168	67,500	172	−15,000	4
27. x. 36	55,500	158	33,000	168	−22,500	10
3. xi. 36	67,500	160	66,000	166	− 1,500	6
5. xi. 36	64,500	164	51,000	170	−13,500	6
17. xi. 36	51,000	168	27,000	168	−24,000	0
8. xii. 36	43,500	164	31,500	168	−12,000	4
8. iii. 37	118,500	148	36,000	160	−82,500	12
15. iii. 37	88,500	160	72,000	160	−16,500	0
22. iii. 37	94,500	161	58,500	158	−36,000	− 3
5. iv. 37	105,000	151	72,000	151	−33,000	0
*12. iv. 37	105,000	130	51,000	127	−54,000	− 3
19. iv. 37	105,000	160	114,000	163	9,000	3

E.D.R.—electrodermal response in ohms of resistance.

S.B.P.—systolic blood pressure in mm. of mercury column (no calculations or conclusions have thus far been made on these data).

* The author acted as subject for all results except this one.

Before we go into the matter in greater detail we must have many more data from a larger number of individuals. It might also be desirable to separate the type of fatiguing stimuli so that in one series only mental work be exacted and in the other continuous physical work be required. From our long experience this separation would not be as necessary as the logic of the situation would demand because the mental experience from our series is very much the same. It is remarkable in this connexion to what a large extent our introspective records tally with the symptoms described by Offner: "In consciousness there appear subjective symptoms, like those accompanying bodily work—at first a mood of indifference, then a disinclination to pursue the fatiguing work, together with the desire for a change. We are 'tired' of this work. Then a feeling of languor becomes evident, a feeling that we can't get hold of things, though we still want to. We feel weary for any kind of work (feeling of weariness). Finally, we feel exhausted, and crave nothing but rest and sleep. Then, not infrequently, headache follows—analogously to the soreness of the fatigued muscle; then restlessness and excitement, heightened sensitivity to impressions (hyperesthesia), especially to noises, nervousness, irritability, ill-humour and liability to passionate outbreaks" ((3), p. 16). Typical analytical records have already been published in my recent book ((7), p. 407).

IV. A SUGGESTED HYPOTHESIS

I wish tentatively to advance the hypothesis that possibly fatigue truly represents a retardation of the activity of the higher cerebral centres and a relapse to controls of the lower centres like the hypothalamus. In that event it might be plausible to suppose the same kind of activity to remain that obtains in the affective area through connexions with the sweat glands and the autonomic nervous system.

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THE INFLUENCE OF SOUND UPON ACHROMATIC CONTRAST

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I. *Introduction* (p. 219).
II. *Method of experiment* (pp. 219–221).
III. *Results* (pp. 221–223).
References (p. 223).

I. Introduction

S. V. Kravkov has investigated the influence of indirect stimuli upon the irradiation of light in the eye (1, 2). This influence is not always of the same character. It may be such as to produce either an increase of the irradiation of white light (the positive irradiation) or a diminution of it. The former was observed when there was strong contrast between the object and its background; the latter when there was weak contrast.

It was this regularity that caused Kravkov to state the hypothesis that stimulation caused by indirect stimuli is distributed unevenly in the centres affected by the direct stimuli (2). And this stimulation is more uneven when there is a greater difference between the levels of the excitation of the adjacent, directly stimulated fields. This hypothesis leads us to believe that strong achromatic contrast increases under the influence of the indirect excitation; whereas weak contrast decreases under the influence of the same indirect stimulus. To verify the above deduction was the object of the experiments described below.

II. Method of experiment

Three paper disks were fixed to the axis of a small motor (colour mixer). Two of them, one black and the other white, had a greater diameter than the third, which was white. A black arc was placed upon the small white disk. The angle of this arc in one series of experiments equalled 180°. A dark ring, appearing on the white disk when the motor revolved, was under these conditions considerably darker than the white disk (a case of 'strong contrast'). In another series of experiments, the angle of the black arc equalled 30°. When the motor revolved under the same conditions a light grey ring was seen, which could but slightly be distinguished from the background (a case of 'weak contrast'). In both cases, the colour of the ring against the white background changed under the contrast action of this same white background on which it was seen. The task of the subject was to match the colour of this narrow grey ring, drawn on the white background, with the colour of the big outside disk. The latter, as is pointed out above, was made up of the white and grey sectors whose size could be regulated by the experimenter.

The rotating disks were seen monocularly by the subject, through a blackened tube. Through it the subject saw only the field traced by the dotted line in Fig. 1. This field was seen through the tube at an angle of 2° (the eye of the subject was at a distance of 65 cm. from the colour mixer).

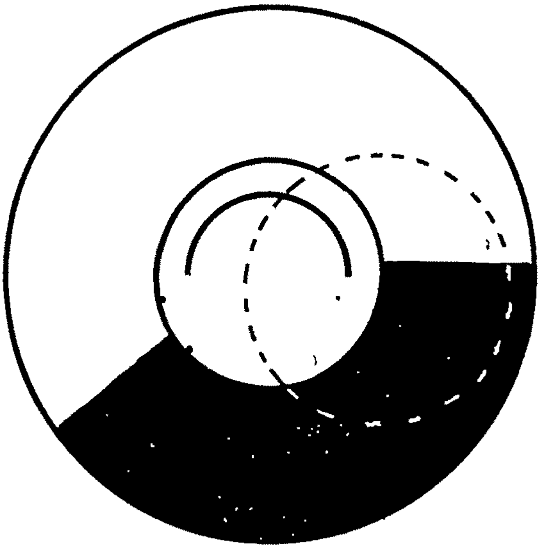

Fig. 1.

The indirect stimulus was the sound of a valve oscillator giving a frequency of 775 c.p.s. and an average loudness of about 70–75 decibels above threshold. The sound was transmitted to both ears of the observer by telephone receivers.

At the beginning of the experiment the subject was told the following: "There are two disks: a white one representing the 'field' and a grey one representing the 'background'. A grey ring appears on the white disk. Look very closely at the colour of that ring and at that of the background.

When after some time I cover the aperture of the tube, you are to tell me if the colour of the ring is identical with the colour of the 'background'; if, however, it is not, then you must tell me whether it is necessary to darken or lighten the background so that its colour may match that of the ring. Please answer in as few words as possible. Does it match, must it be darkened or lightened?"

The experiment was performed under conditions of light adaptation given by two electric bulbs in a room with white walls. The illumination of the disks on the colour mixer equalled approximately 320 lux. At the beginning of the test the subject fixated a black screen through the tube. As soon as the subject placed his head on the head-rest, the motor was turned on and the black screen was removed. The exposure lasted for 20 sec., and afterwards the disks were again screened from the subject's eye and the colour of the background changed in accordance with the subject's indications. This continued until the subject judged that the colours of the ring and background matched, when the size of the white sector on the greater disk was measured and recorded. This was repeated several times. During the intervals between exposures the subject's gaze was fixed upon the white wall of the room.

After the subject had made four matches, there was a 10 min. interval, which was followed by the sound stimulus. The experiment began after 1 min. of adaptation to the sound. The procedure was exactly the same as with the previous exposures. The subject was exposed to the sound for 8–15 min. (depending upon the speed with which he matched the colours). The sound was then stopped and the subject again given a 10 min. rest period, for the after-effect of the sound to pass off before a final control experiment without sound. The values for the white and black sectors of the greater disk were averaged for the two stages. These averages made it possible to judge the change in the value of the perceived achromatic contrast under the influence of indirect stimulation.

In all there were eighty experiments, not counting the preliminary trial tests. There were fifty-six in the first series, where the black arc on the white background equalled 180°; and twenty-four in the second series where the black arc was equal to 30°. The experiments were performed upon nine subjects.

III. RESULTS

In fourteen cases the sound produced no effect at all. In two cases the effect proved of quite an opposite character, contrary to our expectations. In the rest (sixty-four cases) the result corroborated the hypothesis we

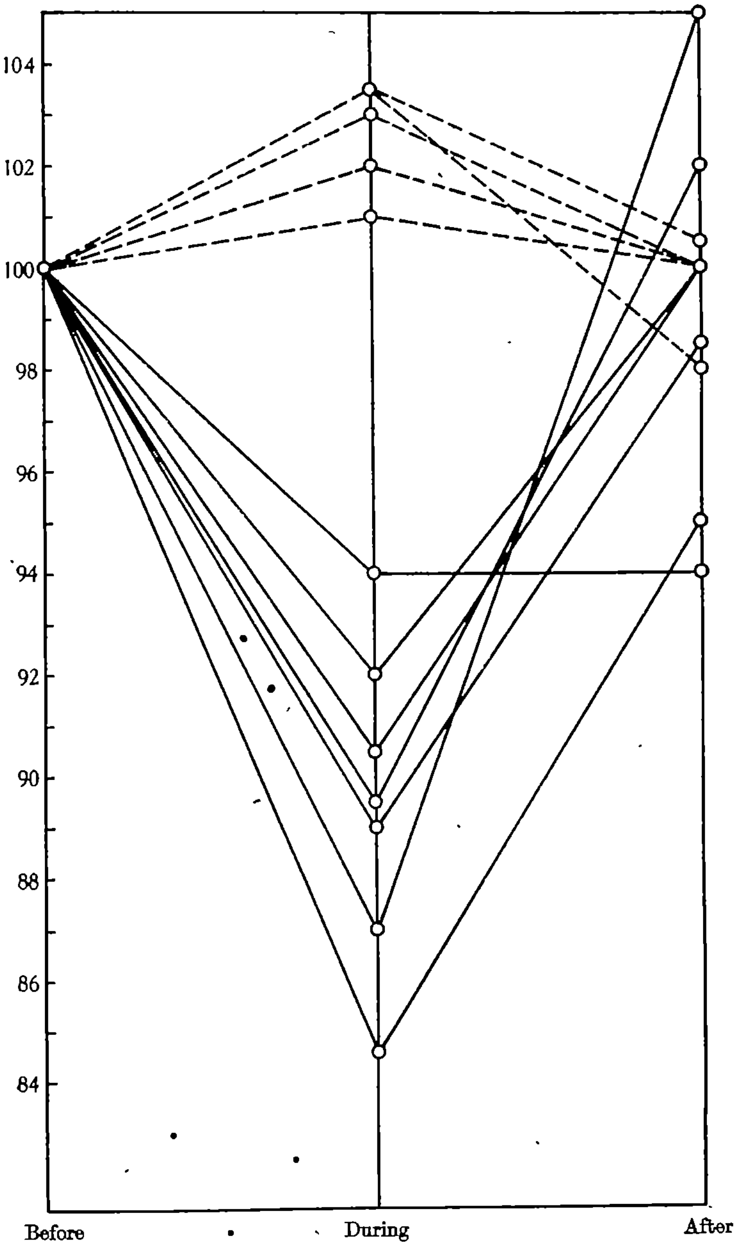

Fig. 2.

set out to verify: i.e. *strong contrast increases under the influence of sound while weak contrast diminishes.*

The average results obtained from all the experiments with all the subjects are given in Fig. 2. The results of the experiments with strong contrast are shown by full lines; those of experiments with weak contrast by dotted lines. The brightness values of the grey of the big disks which the subject considered identical with that of the grey ring on the white disk are marked (in relative values) on the ordinate. The time of measurement 'before the sound', 'during the sound', and 'after the sound' is given on the base line.

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(*Manuscript received* 17 *April*, 1939)

NOTE. Owing to the international situation, it has been impossible to submit the proof of this paper to the author for correction, and for approval of certain slight emendations of the original text.—Editor, *British Journal of Psychology.*

A NOTE ON TACTUAL PERCEPTION IN A BLIND SUBJECT

By J. CREWDSON and O. L. ZANGWILL

Cambridge Psychological Laboratory

I. *Problem* (pp. 224–225).
II. *Method* (p. 225).
III. *Subjects* (p. 225).
IV. *Results* (pp. 226–228)
  (1) *Determination of the tactual straight edge* (p. 226).
  (2) *Differential threshold for straightness* (p. 227).
  (3) *Qualitative aspects of response* (pp. 227–228).
V. *Summary and conclusions* (pp. 228–229).
References (p. 229).

I. PROBLEM

Rubin, in his "Haptische Untersuchungen" (1936), was able to show that the basic dimensions of tactual (haptic) space deviate in certain important respects from those of the visual field. Thus he found that an edge which when tactually explored in a specified manner gives rise to the experience of a straight line is in fact curved in such a way that its centre lies farther from the subject than the two ends. In Rubin's terminology, the haptic horizontal is given by a *plus curved* line ((1936), pp. 288, 290–3, 375).

The degree of plus curvature necessary to provoke an impression of straightness in any given case depends upon a number of factors. One of these, and perhaps the most important, is the axis of the moving member. Thus when the forearm alone is moved, the degree of plus curvature under Rubin's experimental conditions is always greater than when the entire arm is moved about the shoulder as centre (*op. cit.* pp. 292, 375). But with both types of movement the degree of plus curvature which is found to correspond with tactual straightness is always less than that of an arc having as its centre the axis of the moving member. There is thus a sense in which we may speak of *phenomenal regression* (Thouless, 1931) in the tactual sphere.

We have recently endeavoured to repeat Rubin's basic experiment in the Cambridge Psychological Laboratory. But we wished also to obtain some indication as to the responses given by blind subjects under Rubin's experimental conditions. Hitherto, we have been able to work with but a single blind subject, but the results obtained are not without interest. We venture in the present paper to communicate these results in a comparative manner.

II. METHOD

The apparatus used was identical in all respects with that devised by Rubin (*op. cit.* pp. 287–8). The methods which we used conformed exactly to Rubin's own procedure. In the first or *short-arm* (S.A.) method, the subject sat at a table opposite the apparatus in such a position that he was able to rest his right elbow comfortably on the table and to extend his hand to touch the outer edge of the steel ruler with the tips of the index and middle fingers (cf. Rubin, *op. cit.* pp. 287–9). The arm was thus allowed to move only from the elbow. In the second or *long-arm* (L.A.) method, the subject sat slightly farther back with the arm extended in a stretched position and the fingers touching the edge as before. In this position, all movement took place from the shoulder. The subject was instructed to try to maintain a light constant pressure of the finger-tips against the outer edge of the ruler and to move them along it smoothly at an approximately constant speed. At the beginning of each trial the subject placed his fingers at the right-hand edge of the bar and moved them across and back twice. He was then required to give a judgement of *plus curved*, *straight*, or *minus curved*. The constant method was adopted throughout in accordance with Rubin's procedure. No indication was given to the subject at any point as to the correctness of his judgements, and in the case of the normal subjects, the apparatus was of course concealed from view by an arrangement of screens.

III. SUBJECTS

Four subjects co-operated in the experiment. One of the subjects, W. L., had been totally blind for some twenty years. The remaining three were undergraduates with normal vision. Two of these three subjects, B. L. and J. S., were women, and the third, A. St., a man. The average age of the three undergraduate subjects was twenty-one years.

Subject W. L. was a very highly educated woman and most willing to co-operate in the experiment. Her history is briefly as follows: Before the age of about six years, the subject remembers being just able to appreciate forms at close range (e.g. a sixpence held before the eyes). From six to approximately fifteen years of age, she could react only to light, shade and colour. The appreciation of form was no longer possible. For the last twenty years the subject has been totally blind.

IV. RESULTS

(1) *Determination of the tactual straight edge*

The degree of curvature corresponding to a tactual impression of straightness (*haptische Geradheit*) for all four subjects under the two types of condition is given in Table I (columns 1 and 3).

Table I

	L.A.		S.A.	
Subject	Determination of straight in mm.	Difference threshold in mm.	Determination of straight in mm.	Difference threshold in mm.
B. L.	+2·5	1·5	+3·0	3·5
A. St.	−3·0	2·0	+4·0	2·5
J. S.	+5·0	2·0	+9·0	2·5
W. L.	+5·0	2·0	+4·0	2·0

The table brings out the following points:

Firstly, in every case but one the subjective determination of *straight* is a plus curve;[1] that is to say, one in which the ends of the ruler are bent towards the percipient. The actual values show considerable individual differences, but they are all of the order found by Rubin in his original study. It will be observed further that the two values for the blind subject are distinctly on the large side, being exceeded only by subject J. S. in our investigation and by one of Rubin's original eight subjects (cf. Rubin, *op. cit.* p. 292, Tabelle 2).

Secondly, it will be noted that with the solitary exception of the blind subject, *haptische Geradheit* is in fact more curved under S.A. conditions than under L.A. conditions. This result, too, is in good agreement with the findings of Rubin (*op. cit.* pp. 292, 375). At present, however, we do not see our way to accounting for the discrepancy between the results for W. L. and those obtained from the remaining subjects. It may be that some special constancy mechanism is developed in the blind, but further empirical investigation is obviously demanded before any hypothesis of this nature be seriously put forward.

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[1] This solitary instance of an experience of phenomenal straightness being given by a *minus* curve is probably to be explained largely in terms of an intentional process of compensation. This subject (A. St.), who throughout adopted an extremely critical and 'object directed' attitude, remarked during the session in which this series of readings was taken that he assumed that anything which seemed to coincide with the arc of a circle made by his hand must be a plus curve and that he therefore tried to compensate accordingly. A not dissimilar case of minus curvature is reported by Rubin (*op. cit.* pp. 292–3).

(2) *Differential threshold for straightness*

An approximate measure of the differential threshold (*Schwellenwert*) for tactual straightness (as differentiated from curvature) is also given in Table I (columns 2 and 4). It is interesting to note that the differential threshold of the blind subject averages 2·0 mm., which is slightly lower than that of the remaining three subjects, whose average values are 2·5, 2·25 and 2·25 mm. respectively. This comparatively low value of the differential threshold in the case of W. L. does, it is true, point towards a slightly higher consistency of judgement, but the lack of statistically significant differences between the individual results precludes our drawing any conclusions bearing upon the much disputed questions of 'tactile acuity' in the blind. It may be added that the high consistency of judgement shown by W. L. certainly does not correlate with objective accuracy; her average determinations of *straight* are 1·75 and 4·0 mm. greater than the corresponding values obtained from subjects B. L. and A. St. respectively.

(3) *Qualitative aspects of response*

Individual differences in general attitude towards the experimental task were very striking. Two of the four subjects, namely B. L. and A. St., adopted highly critical and analytic attitudes on every trial. These two subjects, it will be remembered, showed a relatively small degree of plus curvature in their determinations of the straight edge. Subjects J. S. and W. L., on the other hand, were far less 'object directed' in general *Einstellung*, and both experienced considerable difficulty in responding to the presented edge as a single, isolated stimulus object. This was particularly the case with the blind subject, who remained surprisingly *naïve* throughout the experiment. These findings appear to indicate that the adoption of a critical attitude favours phenomenal regression to the 'real' character of a tactually presented object.

A second finding of some interest was that the judgements given by the blind subject were apparently determined in an overwhelmingly large number of instances by factors of the order of 'absolute impressions'. She made far less frequent use of 'side comparisons' than was the case with the three control subjects. These features comprised the most salient points of difference between her introspective reports and those of the normal subjects.

The blind subject claimed also that she apprehended what she called tactile 'motion' with the utmost facility. By this, she appeared to mean that she became aware of a change in the curvature of the Rubin edge in much the same manner as she normally appreciates a change in the direction of a vehicle in which she is travelling. In both cases the perceptual interpretation is so rapid as to manifest itself as a virtual 'intuition'. This observation, though no more than an analogy, does indicate that tactile and labyrinthine 'cues' may come to be interpreted by the blind with the characteristic immediacy which we associate with visual apprehension. These questions would seem to suggest an interesting field for future investigation.

V. SUMMARY AND CONCLUSIONS

(1) A repetition of Rubin's experiment on the determination of *haptische Geradheit* was undertaken. The subjects comprised three undergraduates with normal vision and a woman who had been totally blind from an early age.

(2) The results show firstly that the experience of *haptische Geradheit* is typically given by a *plus curved* edge; secondly, that the degree of plus curvature is, as a rule, greater under 'short-arm' (S.A.) than under 'long-arm' (L.A.) conditions of tactual exploration. These results are in good agreement with the findings of Rubin.

(3) The values of plus curvature in the case of the blind subject are distinctly on the large side, being exceeded by only one subject in our investigation and by one subject among Rubin's original eight observers.

(4) There is a reversal of the typical relations between the values obtained under L.A. and S.A. conditions in the case of the blind subject. For this subject alone, *haptische Geradheit* is more curved under L.A. than under S.A. conditions. This finding has no parallel either in Rubin's results or in our own. It is, however, pointed out that further experimental work is required before any conclusions can be drawn.

(5) The average differential threshold for *Geradheit* for the blind subject is slightly lower than the values obtained for the remaining three subjects. These differences are not, however, statistically significant.

(6) Individual differences in general attitude were very striking, and it was found that the adoption of a critical *Einstellung* appeared to favour phenomenal regression to the 'real' character of the tactually presented edge. The blind subject was extremely *naïve* in her attitude towards the experimental situation, and made much use of 'absolute impressions' in effecting her judgements. Certain other features emerging from a consideration of the introspective reports are discussed.

(7) The Rubin edge would seem to offer a useful tool in the investigation of certain perceptual problems presented by the blind. We believe

that further work along the lines indicated in this preliminary study would furnish results of some interest from the standpoint of general psychological theory.

We have to thank Mr K. J. W. Craik for constructing a replica of the Rubin apparatus, and Mr E. G. Chambers for some advice on a statistical treatment of our results.

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TWO CONTRIBUTIONS TO THE THEORY OF MENTAL TESTING

II. A STATISTICAL REGARD OF PERFORMANCE

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From the Institute of Experimental Psychology, Oxford

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I. INTRODUCTION

THE present paper continues the previous one in which a new performance test was described (1), and like that paper the present one is a contribution to the theory of mental testing. Both papers have as their concern Q-technique, the use of *persons* as variables, which I seek to distinguish from r-technique, the use of *tests*, etc., as variables. It is my contention that these two techniques should not be regarded as merely alternative ways of treating one and the same matrix of data (8); on the contrary each offers its own characteristic approach to psychometry. Q-technique can have its own special problems, its own assumptions, principles and objectives, and these can be independent of those involved in r-technique. In the previous paper I offered a demonstration of this by considering some of the conditions under which *ability* might be measured directly as a correlation coefficient, and in the present paper I propose to give another demonstration of the unique part that correlation of persons can play in mental test theory, by showing how persons can be correlated and factorized for the *quality* of their work at a performance test. I seek to reduce performance, in all its qualitative richness, to the neat precision of a mere correlation coefficient.

II. THESIS

Our concern is to be with what the trained psychologist can observe and appraise as he overlooks an individual at work on a performance test. The setting is to be the performance test already described, but the matters to be raised are of general theoretical interest.

Mental testers are often expert at describing the main temperamental and other personality characteristics of the individuals they test, and indeed individual testing is now used with an eye to this wider clinical diagnosis rather than with the exclusive object of supplying a measurement of a person's I.Q. (2). It is now often said that the I.Q. measurement is the least important of the facts that issue from testing a person. At present, however, there is no systematic way of dealing with this wider diagnosis; the Binet has been subjected to some such treatment (3), and the Rorschach test has claims to consideration in this connexion (4), but for the main part the psychologist's observations are of an incidental nature, which he describes in the form of 'case notes'. It is in no way suggested that these literary productions should be replaced by statistical treatment of the kind to be considered in the sequel, but it seems worth while asking whether a method can be devised which will take due account of these incidental observations and at the same time make them amenable to comparative and systematic study.

The psychologist may direct his attention to the subject's general demeanour, his dress, attitudes and the like, or to the intimate details of the performance itself. Much will be learnt about the individual's personality from both these sources, which are far from unrelated, for even the colour of a subject's hair may help us to appreciate why he swoops on the performance pieces like a hawk or dilly-dallies with them like a cat playing with a mouse. But for the test to have any special claim to attention it must supply its own quota of information about the person tested, information which depends intrinsically upon the test material itself. In the case of a performance test this has hitherto usually consisted of a bald statement about the probable I.Q. of the person.

When an I.Q. is measured, little direct account can be taken of the details of a person's performance. If it is a *timed* performance test that supplies the measurement, the individual may work over-cautiously, slowly, and with lack of foresight or system—indeed his work may involve a thousand and one qualitative characteristics of this kind; and yet all that the I.Q. is directly concerned with is the time it takes him to arrive at the required solution. This does not mean, however, that these qualitative characteristics are completely overlooked in the time score, for clearly at least some must vary in direct proportion to the time it takes the individual to solve a task. He cannot work inattentively, unsoundly, and with lack of judgement, and at the same time solve the task quickly; and the person who solves it slowly cannot usually have done so aptly, and with efficient constructions. The time measure thus embraces some of the qualitative aspects of performance. The trouble is that we do not know which, and much is taken for granted that should be explicitly attested, and important qualities such as perseverance, initiative and the like might be very unsatisfactorily accounted for in the time measures. Clearly these myriad qualities of performance, in all their diversity and complexity, should be investigated on their own account, if possible *in situ*. It is well known that performance tests afford an excellent medium for laying bare these qualitative characteristics: One can almost see what the person is thinking about by looking at his finger tips.

The thesis that I am to develop in the sequel is that performance, smooth-flowing stream though it may be, can be regarded as a population of innumerable response items or qualities of response. These, with all their interconnexions, can be evaluated for their relative *significance*, or importance, or dominance, in the individual's work as a whole. It is thus permissible to select a sample of these items and to quantify them item by item for their relative importance in the individual's total behaviour, and thereupon to correlate one person with another for a given sample of these response items. Two persons performing a test with the same kind of qualitative picture, therefore, would correlate positively. A number of persons could be intercorrelated, and factorized. The implications of such a thesis and technique, and the purposes to be served by this factor analysis, are considered in the sequel.

III. THE BASES OF QUANTIFICATION

All the qualities of response that appear in the course of work at performance tests, such as foresight, perseverance, initiative, trial-and-error thinking, confusion, shape dominance, imaginativeness, motor agility and the like, can be quantified in at least two distinct ways which are methodologically as different as chalk and cheese. In one case the qualities may be quantified in their *immediate* context, which is the concern of Q-technique, and in the other the qualities may be considered in more *general* contexts, each in its own right as a possible ability, and in this case r-technique is involved. In the former case a basis of quantification, in terms of which different qualities may be compared, is the *significance* of each quality in the particular performance, or kind of performance. The concept of *significance* is the major theme in the present paper; it is the outcome of special needs, is peculiar to Q-technique, and as such the concept I think may be considered to be new to systematic psychometry, although something of the kind has long been implicit in any clinical regard of mental testing.

It has already been stressed, in the previous paper, that the premisses and assumptions which underlie our testing practices and theories are apt to be overlooked, and it is necessary to attend to these for the purpose of distinguishing between Q- and r-techniques. Thus, in r-technique each quality is a variable, and quantification pertains to a statistical population of persons, assessed or measured *for amounts of that particular quality*. If our concern is a quality such as perseverance, to take this as an example, we appraise or measure each person for his degree of that quality, and usually there are ideas at the back of our minds about the kind of frequency distribution to be assumed or expected. Our essential interest is with perseverance in a general way: in particular cases the individual may be bored, exhausted, or discouraged, and show little perseverance; but r-technique is only concerned with his average or more general *capacity* for that quality. When we test him for this quality we try to ensure that he will have every chance to do justice to himself; we make the conditions propitious, and remove any militating influences. And we would seek to make any tests of perseverance as free as possible from contamination with other qualities. That is, whether we merely estimate the quantities or objectively measure them, the assessments are based on the premiss that the conditions for all individuals are equal or controlled with respect to all other qualities except the one under consideration at the moment. Every quality, as far as possible, is quantified with respect to controlled conditions, or else, what is the same thing in effect, the quantities are average or general amounts.

Quantification in r-technique, then, involves a separation of the qualities from any particular contexts; each quality is separately quantified under controlled conditions, or is an average of innumerable events. A complete sample of individuals, one by one, may be quantified in this way for any number of different qualities—for perseverance, synthetic thinking, confusion, and the like. Finally the arrays for these different qualities may be correlated and factorized, with the object of seeing whether the quantities for the different qualities vary proportionately, or display some kind of orderliness or system. By far the greatest part of current mental testing is founded on quantification and conceptions of the above kind—all the factors of Spearman, Thurstone and Kelley have their origins in r-technique, as have the more empirical formulations of a Binet, Terman, or Rorschach.

The many qualities that we can observe in an individual's work at a performance test are quantified, in Q-technique, under very different conditions. We now attend to more *particular* events, to the work of one person only, if need be, in such a test; or we might concern ourselves with his work in a particular kind of performance test, such as that constituted by the twenty tasks of the new performance test described in the previous paper (1). The twenty tasks of that test series are very similar in material and form, and it is possible to focus attention on the individual's work in all twenty, on the ground that essentially the same performance is mirrored in each. In Q-technique we deal with the person as variable, and the many qualitative features of his work have to constitute a sample that has been drawn from a statistical population of such qualities pertaining to the individual's work. Moreover, a common basis has to be found for mutually comparing these qualities *in situ*, in their particular context, just as they appear in an individual's performance. The basis might be qualitatively different for each person, but the many different qualities or responses that comprise one person's work must be quantified on a common basis relative to each other; we are indeed to contemplate the individual differences *of these qualities* in a person's performance, for some characteristic they all have in common.

We should ask, first of all, in what sense a number of responses and characteristics of work at a performance test can be said to constitute a statistical population of qualities. Certainly countless qualities may be involved, but they are connected in a complex and ever-shifting causal and logical system. Performance shows main lines of endeavour and direction towards ends. It is an irreversible stream of behaviour, flowing with more or less directness into the final sea of all tasks that are ended and done with. Yet all this flow of cause and effect, of stimuli and response, has to be conceived of as a mere statistical heap of qualities, a jumble of unrelated units! But, as I tried to show in the previous paper for similar doubts about the conditions under which *tests* could be said to constitute a statistical population, the units that we deal with in psychology need not be conceived of as unrelated, so much as so complexly and completely interconnected, so highly ramified in their interrelations, that only a statistical regard can do justice to them. When the relations between the items are so innumerable that the theory of 'errors' can be applied to their quantities, the statistical regard is not without justification. It will be admitted that my chest measurement is not less real for regarding it as dependent upon innumerable small influences, depending upon heredity, exercise, food, habits and the like (5). No less tangible is the stream of behaviour in performance when I regard it for convenience as a statistical universe of items, from which I can draw samples.

New brooms, however, should be fitted with new handles, and performance tests can be made which are specially fitted to this statistical regard of performance. They can entice the individual into displaying a rich variety of behaviour; and special procedures can be devised, new forms of tests, which will make it less and less unreasonable to regard the qualities of a person's work as items in a statistical population. In the sequel, for example, I appraise the work of an individual in twenty comparable tasks, and in this way achieve something that is at least a pale reflexion of the required conditions.

As for the common basis of quantification, I put forward the concept of *significance*. The different qualities of a person's work can be quantified with respect to their relative significance in the test situation they comprise. Some characteristics of a performance are more important than others; they lead rapidly to a solution, or bring the individual up against a blank wall and stop his progress. Others seem to colour the whole work, and are by far the most significant responses in the situation—as, for instance, when the individual is in a dither of indecision, or when he suffers from temporary *aboulia*. Significance is in no essential way dependent upon the *frequency* with which a response may appear. We can readily count whether one characteristic appears more often than another, and some undoubtedly are significant because they occur repeatedly in a test performance; but the most significant response item or quality in a performance may make its appearance for an instant only, and on one occasion only. A flash of insight, for example, might be of this nature. The significance of a quality, then, is far more than frequency of occurrence. It is an evaluation, made in the light of the total test situation, and relative to that situation, of the immediate appearances and their underlying causes and consequences. The concept requires, no doubt, further elaboration and illustration, but that can be attended to later. Meanwhile a question remains, concerning the *units* of quantity. We might assume, to make a beginning, that the quantities for a large sample of qualities that have been appraised for their significance, are distributed on a 'cocked hat' frequency curve. Only a few items will be very significant in a performance, and only a few extremely insignificant: most will be intermediate in amount.

Again, however, technical resources can help us, and this last assumption is not really needed. Nor does it do justice to the facts, for significance can have a positive and a negative direction, and *most* qualities would seem to be of little significance in a performance. The technique I have to describe in the sequel makes it unnecessary to postulate an arbitrary frequency distribution of quantities—it again owes its efficacy to the use of twenty comparable, approximately equal, tasks. Quantities are scaled from positive values for the qualities that are significant in helping the performance towards its proper objective solution or end, to negative ones that are no less significant, but in blocking and delaying the solution. Most qualities are usually intermediate: they neither help the solution forward very much, nor hinder it.

Finally, upon whom or what are we to depend for the quantification of qualities of significance? I can think of no test instrument, other than the psychologist himself, which could render an account of the required quantities. No instrument or test can tell the watchmaker that the balance wheel is the most significant part of a watch; and no tools or tests can tell the physicist that the characteristics of a valve are the most important regulators of a wireless set. Knowledge of this kind is technical, part and parcel of the craft or science involved, and it comes only from the craftsmen or scientists concerned. Similarly in the case of a performance test, only the psychologist can decide upon matters of significance. This by no means implies that appraisal for significance is merely a subjective matter, for any one psychologist's appraisal can be checked against the appraisals of others. Moreover, in so far as the onlooking psychologist draws upon his technical knowledge, his estimations can be as expert and as scientific as the main body of information upon which he has formed his judgements. He merely mirrors the science of which he is a member. With due safeguards, common in any science, I see no reason why we should not depend upon the psychologist for these appraisals of qualities for the significance in a test situation. We grant the judge his expert eye for the fine points of a horse; no less a respectability should be granted to the clinical psychologist with an expert eye for the finer points of a test performance.

The stage is now set, then, for an illustration of this application of Q-technique to performance. There is to be a frank acceptance of the psychologist's role as onlooker. I myself will act as observer in the example to be described, and my appraisals will be governed by the kind of systematic psychology that I think is fairly current amongst mental testers. Attention is to be paid to responses which are intrinsic to test performance. There is to be a rich array of performance to observe, the responses and qualities of which can be regarded as a statistical population. Any sample drawn from this population of response items is to be quantified with respect to the relative significance of each of its items in the performance as a whole. And for several reasons, amongst them considerations of reliability and validity, a large number of comparable tasks are to be evaluated. Using the sample of qualities as a population, and a number of persons as variables, correlations are to be calculated and factorized. The objects to be served in this way are to be referred to in the sequel.

IV. RESPONSE ITEMS AND METHOD OF APPRAISAL FOR THE NEW PERFORMANCE TEST

The above theoretical conceptions find a ready practical illustration in terms of the new performance test (1). It will be recalled that the test consists of twenty tasks, each involving a number of pieces which have to be placed in juxtaposition so as to make a shape which, as nearly as possible, is a match of the given standard shape. I propose making use of data gathered from the forty students already referred to in that paper, and it will be remembered that I use these students purely for illustrative purposes, for the appraisals could as readily have been made for any subjects capable of handling the test material.

As a result of preliminary observation of students I drew up a list of response items and qualities that I could readily observe, or infer from observation, whilst an individual performed these tasks. From this *ad hoc* list I selected sixty at random; this sample is given in the Appendix. Each quality was carefully defined beforehand, and only the short titles are given in the Appendix. No. 1 item, for example, was defined as follows:

> Tendency to shift pieces from one position to another without due cause; without looking at pieces they are picked up and then vaguely put down again; tendency to grasp impulsively at pieces, and to replace them randomly.

It was expected that I keep these meanings in mind and report unvaryingly upon them, but I could at any time refresh my memory by referring to the full list of definitions—which I always did prior to beginning an appraisal. The sixty short titles were cyclostyled, so that a clean copy was available for appraising each individual. The items are merely a sample of the many responses that can be observed or inferred in the course of an individual's performance, and each could be subdivided in most cases, if need be, so further swelling the total population to be considered. But a beginning has to be made somewhere, and this sixty is quite adequate for the present purpose.

It is important to notice that the twenty tasks (A to T of the previous paper) are very similar, involving similar problems, goals, and material, and making similar demands upon the thinking and manipulative abilities of the individual. It is as though the same task were presented to him, in a modified form, on each of twenty occasions. For this reason the sample population of responses can apply to each of the tasks in turn, or to all twenty as a whole. This is of considerable practical assistance, because it is possible to appraise each task in turn and to sum the results for all twenty, thereafter using this summation as the required evaluation of the items for their relative significance in the individual's performance. This not only makes for reliability and validity, but is indeed a means of arriving at reasonable frequency distributions for each person, and also gives an appraisal that more truly represents the kind of performance involved in the test as a whole. Our concern is still with a particular event, but we repeat it so as to appraise it the better.

With this in mind it is most convenient to appraise an individual's work in the following standard manner: As the subject works at the test the observer sits in good view of all the performance, and goes through the cyclostyled list of items, one by one, placing a *tick* against those which are significantly displayed in the performance, and a *stroke* if they are conspicuously absent in conditions otherwise calling for their display or if they are otherwise negatively significant. Two ticks or strokes might be given to a task on occasion, if the quality concerned seemed to be extremely significant. The practice is followed of first observing the individual's work for half a minute or so, and then beginning the appraisal. At the conclusion of the twenty tasks a count is taken of the number of ticks or strokes (or difference between them) adjacent to each response item, and this number is thereafter regarded as the score for significance of that item, for the individual concerned. It is important to notice, as I indicate later, that these scores need bear no relation to a person's capacity or ability for the qualities in general. Items which are not conspicuous in either direction, or which are neutral, tend to have zero significance. It is not that these items are absent in the person's performance, for by assumption and fact they are present—they appear in some or most of the twenty tasks, if not in all. They are merely responses which are scarcely of moment in the task, of little apparent consequence in it. A response item which is neutral for nearly everyone, can be highly significant in another's performance. Thus indecision, which is of no great moment in the work of most subjects, can on occasion completely colour a person's work, being by far the most significant quality of it. It is usually not difficult to decide when a quality is significant in a performance, either positively or negatively, because it marks a step forward towards a solution, or markedly delays or blocks that step forward. Indecision, confusion, excessive timidity, trial-and-error procedure, extreme competency, and the rest of the sixty qualities, any of these can colour every other with its own character, or stand out above them as the most striking and obvious feature of the individual's work.

V. ILLUSTRATIVE EXAMPLE

Data were gathered from the forty students already alluded to, using the above technique, all appraisals being made by myself. The facts to be considered consist of an array of scores for each student, each array consisting of the summed scores (ticks or strokes) given to each of the sixty response items or qualities listed in the Appendix. Consideration of these data should afford a good example of the whole matter discussed above.

Following each quality in the Appendix, I give the scores for significance obtained by each item as the result of appraising *one* student's work. This is one of the forty arrays we have to consider. Each student's array is very similar, all having a mean of approximately nought, a standard deviation of about 4 units, scores ranging from about +12 to −12 in each case, with a few students of very colourful performance having a range greater than this, from about +16 to −18.

Table I may be of interest. It gives the grand distribution of scores obtained by the sixty items, for all forty students. There are 2400 appraisals in all, the mean score being 0·528 ± 0·097 (S.E.), and standard deviation 4·74 ± 0·068 (S.E.), the distribution having the familiar 'cocked hat' shape. There has apparently been a tendency for me to give relatively fewer scores adjacent to zero (−1, 0 and +1) than might have been expected, as is shown by modes at +2 and −3; this means that when I have been in doubt about the significance of a quality I have tended to give it a mark (tick or stroke) rather than not.

The simplest way of dealing with these arrays of scores is to correlate them, the variables being students, and the population common to these being the sixty response items or qualities. A typical set of intercorrela-

Table I. *Distribution of scores given to the sixty response qualities, for all forty students*

Score (ticks)	No. of times this score is given to qualities	Score (strokes)	No. of times this score is given to qualities
16	4	− 1	138
15	3	− 2	182
14	2	− 3	230
13	1	− 4	155
12	12	− 5	106
11	19	− 6	54
10	40	− 7	49
9	51	− 8	21
8	27	− 9	26
7	48	−10	12
6	95	−11	4
5	151	−12	1
4	170	−13	3
3	219	−14	5
2	224	−15	
1	158	−16	1
0	177	−17	7
		−18	5
			Total 2400
			Mean 0·528
			S.D. 4·74

tions for sixteen of the students is given in Table II. I have not considered it necessary to provide the intercorrelations for all forty students—a total of 780 correlations in all—because Table II is quite sufficient to illustrate the way in which all such data can be treated.

The correlation table has been arranged so as to bring out immediately the similarities and differences between the students. It requires no knowledge about factor methods to note that the students fall into three groups, students 1–4 into group I, 5–12 into group II, and 13–16 into group III, the students of one group correlating more amongst themselves than with students of the other groups. Positive correlation between two persons means that their performances are *qualitatively* similar, and the higher the correlation the greater the similarity. Negative correlation means that their performances have opposite qualities to the fore: what is positively significant for the one person is negatively for the other. If two persons do not correlate significantly their performances bear no qualitative resemblance to each other; they involve the same qualities, but their significance in the one person's performance is random compared with that of the other person's. The correlations in Table II show that the larger proportion of the students (nos. 5–16) intercorrelate positively, and to the extent of the correlations shown their work must

Table II. *Showing product-moment correlations (Q) for sixteen persons* (1–16) *for a population of sixty response items. The persons are arranged in order of types, I, II and III respectively.*

	Persons	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Type I	1	—	380	466	224	060	069	309	-011	073	-121	201	035	-550	-380	-240	-314
	2		—	335	316	069	081	-050	106	080	-002	183	118	-462	-551	-600	-518
	3			—	291	-160	022	051	-003	-084	-031	008	053	-334	-427	-511	-444
	4				—	090	015	-033	-065	033	-095	064	075	-221	-317	-440	-392
Type II	5					—	490	523	432	471	416	209	111	-082	314	250	-093
	6						—	300	267	284	298	141	233	018	202	121	031
	7							—	400	310	327	071	162	070	332	340	125
	8								—	241	331	095	205	071	193	405	088
	9									—	505	274	183	111	093	213	050
	10										—	231	255	311	292	336	214
	11											—	302	108	157	209	063
	12												—	301	277	250	099
Type III	13													—	520	505	493
	14														—	640	564
	15															—	611
	16																—

display qualitative similarities. Students 1–4, on the other hand, whilst correlating positively amongst themselves, correlate negatively with students 13–16. On the average there is very little significant correlation between students in group I and those in group II.

The three groups of students can be studied by averaging the scores given to each response item *for the students of a group*, so arriving at an average array which represents students of that group. The three averaged arrays can then be compared; in particular it will be most interesting to note which of the response items are most significant (positively or negatively) in the three groups, and which items show the biggest difference in significance in one array compared with another. Thus, students in group I were chiefly notable for the following qualities in their performance:

Capable space-sense ability, methodical, deliberate, observant, capable, rapid, logical-minded, makes little use of trial-and-error, uses clues efficiently, no motor disabilities, imaginative use of material, analytical, helpful use of abstraction and generalization, colour clues are used first, not dominant for either shape or form.

Students in group II were notable for the following qualities:

Much use of trial-and-error, synthetical, rapid, practical, medium space-sense (or poor), tendency not to use colour clues first, shape dominant, given to impulsive over-moving, logical, accept inexact constructions.

Students in group III gave the following significant picture:

Predominant use of trial-and-error, little dependency upon available clues, little generalization or abstraction, inexact constructions accepted, stiff in movement, unimaginative use of material, medium or poor space-sense, slow, unobservant.

Persons in group I clearly appear to be most capable, and those in group III least.

VI. Factor analysis

The above empirical analysis can be represented precisely in factor form, and of the various factor techniques that might be employed I prefer the following: Persons of group I may be regarded as of a *type*: they satisfy the *theorem* of two factors amongst themselves. Let this type be represented by a factor I. Similarly each of the other two groups can be represented by factors II and III respectively. Table II shows that these factors tend to be *oblique*, that is, they correlate with one another—in particular factor I correlates negatively with III, but is orthogonal with respect to II. I suggest elsewhere (6) that factor representation by, if necessary, oblique factors, is the first and essential purpose of factor study, and that analysis of correlations into fractional orthogonal factors serves a secondary purpose, or merely helps one to arrive at the required oblique factors. Factors such as I, II and III are *unanalytical*, in the sense that they involve no fractional regard of the available correlation.

The factor-saturations of the students themselves are calculated in terms of students of a type *only*. Thus, considering first the four students of type I (persons 1–4), their factor-saturations are calculated relative to each other by use of the Spearman formula 16A[1], with the results given in Table III (column (i)). Next the eight students of type II have their factor-saturations calculated, relative to themselves only, from the same formula, with the results shown in column (ii). Finally the same treatment is given to the four students of type III, with the results shown in column (iii).

Table III. *Showing oblique factor-saturations for the sixteen persons 1–16, derived from Table II*

	Factor-saturation (Spearman formula 16A)			Factor-saturation (Modified Spearman 16A)[1]		
Persons (1–16)	I (i)	II (ii)	III (iii)	I (iv)	II (v)	III (vi)
1	624				122	-489
2	600				128	-713
3	650					-573
4	439					-455
5		736				
6		530		076		111
7		550				275
8		516				232
9		611				150
10		644		-096		385
11		328		180		173
12		367		117		305
13			650	-664	192	
14			792	-718	429	
15			805	-761	491	
16			745	-716	121	

But for comparative purposes it is also useful to have the factor-saturations of each person in each of the *other* factors, and these are given in the remaining columns of Table III. They are calculated from a modified form of formula 16A,[1] in which persons of a type are used as 'reference

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[1] The well-known formula is

$$r_{ag} = \left\{\frac{A^2 - A'}{T - 2A}\right\}^{\frac{1}{2}}.$$

(From Spearman, C., *The Abilities of Man*, Appendix, p. xvi.)

The modified form is

$$r_{ag} = \left\{\frac{A^2 - A'}{T}\right\}^{\frac{1}{2}},$$

where a is the student whose saturation is required, g is the factor, A is the sum of the correlations between a and the reference value students, A' the sum of the squares of these correlations, and T is the sum of the intercorrelation between the reference value students for a matrix in which both halves are entered.

values'. The factor saturation of a person in any *other* type is obtained by adding him to the matrix of 'reference values' for that type; never more than one person is added at a time, and he is removed from the reference matrix as soon as his saturation has been calculated. A more detailed account of this typology is given in the paper previously mentioned (6), in which I examined Prof. Burt's recent important papers on correlating persons. It may be observed in passing, however, that the types here contemplated are not necessarily unique or distinct—all in fact involve the same qualities, and one type may correlate with another. Table III shows that type II is almos tuncorrelated with the other two; but types I and III are negatively related, as columns (iv) and (vi) show.

Using a short method that I need not go into here it is easy to determine into which type, if any, the rest of the forty students would fall if they were added to Table II. Of the forty it appears that seven belong to type I, ten to type III, and twelve to type II; four might as readily be placed in types I or II, and the rest (seven) equally well in either type II or III.

With a larger population of qualities a finer differentiation into types might be expected; just as with a smaller population the types may be less clearly distinguished—type II, for example, might then be swallowed up into types I and III.

There is little that is surprising in the three types: some are superior performers (type I), others average (type II), and others inferior (type III). As might be expected, there is a tendency for the student to be less significantly capable, less observant, less imaginative, etc., as we pass over from type I to II, and from II to III. The qualities which account most significantly for the differences between the types can be distinguished in detail: thus types II and III tend to be synthetical workers, constructing their copies of the standard shape in terms of what is given on the pieces rather than in terms of explicit analysis of the standard; type I students, on the other hand, take a more analytical approach to the tasks. Type II are shape dominant, whereas type III are either colour dominant or else negligent of colour altogether; type I are dominant for neither, but make use of both colour and shape appropriately.

It is important to notice that persons of type I would undoubtedly fare better than those of type II in performances of a *controlled* kind, in which all alike, for example, are called upon to work synthetically only. Type II individuals, when faced with a task that permits of either a synthetical or an analytical method of work, appear to take the line of least resistance, and work synthetically. But this is merely a preference, and by no means does it imply that they are the most *capable* synthetical workers. Similarly in the case of colour and shape dominance: under controlled conditions for these qualities type I persons might show themselves to be pronouncedly dominant in one or other direction. That is, the *significance* of a quality in a particular situation need bear little or no direct relation to the controlled measurement of that quality. No better example could be afforded of the difference that exists between Q-technique and r-technique than this divergence between qualities considered from their *significance* and ability or capacity aspects respectively.

VII. CONCLUSION

It is possible, and I think permissible, to regard a person's work at a performance test as a statistical heap of qualities which can be compared for their relative significance in his performance as a whole. In this way some account is taken of the many details of a performance, in their immediate or particular context.

My objectives have been primarily theoretical, but the above application of Q-technique to mental testing can be put to practical use, and might do something to help those who have long drawn attention to the need for systematic study of the qualitative details of performance. When an I.Q. is measured by use of a performance test, the baldness of the measurement could be covered by a detailed array of the qualities that enter into it, or that it conveniently overlooks. This would greatly facilitate the orderly and intimate comparison of individuals. Moreover, it is in no way essential or necessary to work with large numbers of persons, for sound correlational comparisons can be made with as few as a pair of twins, or one or two families of siblings. For work on twin resemblances, or for any detailed comparison of individuals, the statistical view of performance can be invaluable.

On the theoretical side I have just skimmed the surface of a topic that requires very much more analysis and illustration. For the present, however, in the two papers of which this is the second it has been my purpose to demonstrate that Q-technique, the use of persons as variables (as distinct from r-technique, the use of tests or qualities as variables) has its own demands to make in mental testing, and its own contributions to offer. These two techniques are the pillars of systematic psychometry, and each has its own foundations; each has its own objectives, involves its own premisses, and dictates its own requirements. It may well be possible to examine any given matrix of data *either* with respect to persons, *or* tests and qualities, as recent papers by Burt (7) seem to indicate; but in the two techniques under discussion, two different matrices are involved, and these are not interchangeable with respect to each other. Both matrices maintain the proper distinctions between a variable on one hand, and a statistical population on the other. And the data that go into the one technique can usually in no conceivable way enter into the other—for different objectives and premisses are involved. In the first paper the distinction between a variable and a statistical population led us to contemplate a new way of measuring ability, for which a new form of test had to be devised; ability was measured in a systematic way, independently of any statistical regard of persons as a population. In the present paper the same distinction has led us to conceive of a person's performance in a test as a statistical heap of items, which can be appraised for their mutual *significance* in his performance. This concept, born out of the present application of Q-technique, pertains to the immediate psychological situation; the analogous concepts that have long grown into maturity in r-technique, such as those of ability and capacity, refer instead to average, general, or controlled conditions.

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APPENDIX

The following is the list of qualities used in the above study; they are selected at random from a larger list. All refer to responses to the performance tasks; only short titles are listed. (The number *following* each quality is the score given to it in the appraisal of *one* student's work. See p. 239.)

1. Impulsive over-mover (10).
2. Timid use of material (−4).
3. Slow worker (−5).
4. Inhibited (0).
5. Hesitant use of material (−4).
6. Deliberate (2).
7. Predominant use of trial-and-error (3).
8. Little dependency on available clues (−3).
9. Works to a plan (1).
10. Systematic (−3).
11. Tries pieces at random in appropriate conditions (2).
12. Colour clues used first (2).
13. Builds shape with little use of colour clues (11).
14. Analytical (2).
15. Synthetical (10).
16. Capable and competent constructions (4).
17. Observant (4).
18. Good use of judgement (1).
19. Rapid worker (11).
20. Fluent (7).
21. A feeling for completeness (4).
22. Imaginative use of material (3).
23. Stiff, clumsy (−3).
24. Ponderous (−8).
25. Anxious (3).
26. Repeats errors (−4).
27. Compulsive (−4).
28. Perseverates (0).
29. Accepts inexact constructions for shape (−6).
30. Accepts inexact constructions for colour (8).
31. First regards available pieces for hints (−1).
32. Logical (−2).
33. Reasons (0).
34. Fits pieces without looking for a clue (3).
35. Accepts inexact constructions at conclusion (0).
36. Makes use of helpful abstractions (1).
37. Makes use of disturbing abstractions (4).
38. Practical (2).
39. Reserved in movement (−6).
40. Leaves well alone (−1).
41. Matter-of-fact (−3).
42. Dominant for shape (9).
43. Not abstractive for shape (1).
44. Competent space-sense (2).
45. Decisive (5).
46. Objective for size (0).
47. Steady output (6).
48. Fluent use of hypotheses (−2).
49. 'Fixed' ideas (1).
50. Speculative (2).
51. Wants identity (−2).
52. Judges aesthetically (−4).
53. Judges logically (−5).
54. Capable speed (6).
55. Over-critical (−3).
56. Inaccurate (−1).
57. Confused (−1).
58. Impulsive (5).
59. Talkative worker (−3).
60. Too-careful (−4).

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ANSWER-PATTERN AND SCORE-SCATTER IN TESTS AND EXAMINATIONS[1]

By DAVID A. WALKER

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II. *An examination of the results of the Scottish Mental Survey* (pp. 249–253).
  (1) *The verbal test* (pp. 249–251).
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III. *The relation of difficulty level and skewness of answer-pattern-differential* (pp. 254–255).
IV. *The variation of fit of answer-pattern-differential and score-scatter* (pp. 255–259).
  (1) *Criticism of h* (pp. 256–257).
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V. *Summary* (pp. 259–260).
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I. THE PREDETERMINATION OF SCORE-SCATTER BY ANSWER-PATTERN

THE results obtained in an examination are often displayed in the form known as the score-scatter, which shows how the scores of the candidates are distributed over the possible range. From such a distribution the examiner can determine the average score, the standard deviation, and the skewness of scores, and from these figures may draw his conclusions on the nature of the population tested. But, as every examiner knows, the score-scatter is determined to some extent by the nature of the test applied; it is the product of the interaction of test and population, and as such it reflects the characteristics of both test and population.

The nature and extent of the dependence of score-scatter on the type of test used can be measured only when the phrase 'type of test' has been translated into terms as definite and measurable as those defining score-scatter. In previous papers in this *Journal* (1, 2), the author has suggested that the essential features of a test where the items carry unit score are best summarized in its answer-pattern, which is the table of the frequencies of correct answers to the items, when these have been placed in ascending order of difficulty, the number of candidates being the first frequency given. It was shown in these papers that where the population tested is of the usual type, it is possible to predict from the answer-pattern the characteristics of the score-scatter with a fair degree of accuracy. The present paper contains a study along these lines of the results obtained in the Mental Survey of Scotland in 1932. This is followed by an account of further researches into the relation of answer-pattern and score-scatter.

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[1] Read in part before Section J (Psychology) of the British Association at Nottingham on 7 September 1937.

II. AN EXAMINATION OF THE RESULTS OF THE SCOTTISH MENTAL SURVEY

In June 1932 the Scottish Council for Research in Education conducted a mental survey of an age group in Scotland, in which all the available children born in 1921 were tested by group intelligence tests. There has already been published a full report (3) giving details of the tests used and the results obtained, therefore reference is made in this paper only to those aspects directly concerning the present investigation, and one or two rather lengthy tables of figures of which use is made here are not reprinted from considerations of space.

In the form finally used for the survey the test consisted of two parts, a verbal part of seventy-one items, and a pictorial part of nine items. For our purpose these parts may be considered separately and referred to as the verbal and pictorial tests. It may be as well to point out here that the 1932 Mental Survey Test subsequently published (4) is not quite the same test, minor alterations having been made in the light of the results obtained. One of the alterations is rather interesting from the point of view of this paper; it is the re-ordering of the items in more accurate ascending order of difficulty, a procedure possibly suggested by the researches reported in these papers.

(1) *The verbal test*

This test consisted of seventy-one independent items, practically all of them carrying unit score. The score-scatter was obtained from the papers of all the candidates, a total of 44,210 boys and 43,288 girls. The answer-pattern, on the other hand, was derived from the papers of 500 boys and 500 girls, the cases being selected at random by the marking committees. The random nature of the selection was tested and found satisfactory. This conclusion is strengthened by a test made by the present author; the answer-patterns of boys and girls were found to differ only to the extent expected through random sampling. The test used for this purpose was Pearson's test of Goodness of Fit, as revised by Fisher (5).

The answer-pattern obtained by combining these sets of data is shown in Fig. 1. The figures from which it is constructed are to be found in the report (⑧), pp. 87–8). This type of answer-pattern might be called steep, in that the items progress smoothly from extreme ease to extreme difficulty, covering the whole range. The answer-pattern-differential, which is the table of differences between adjacent items of the answer-pattern, is therefore roughly a rectangular distribution, i.e. each value of the abscissa has the same frequency. When the values of the answer-pattern-differential have been grouped in tens, as was the case with the scores, the coefficient of skewness as calculated by the third moment formula is found to have a small positive value 0·20. (The application of moment formulae to these distributions has been discussed in previous papers.) On the basis of the theory put forward in these papers the score-scatter should have similar characteristics; in particular its skewness should be slight.

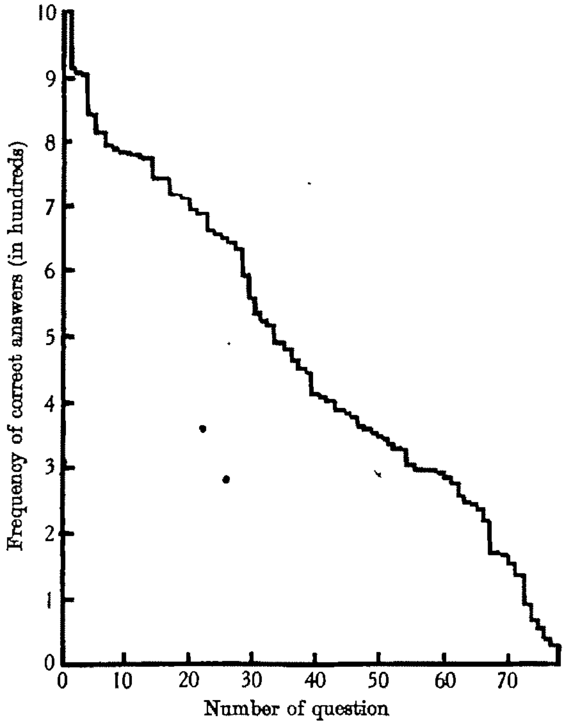

Fig. 1. Answer-pattern of verbal test

The score-scatter obtained from the total population of 87,498 candidates is shown in Fig. 2, the scores being grouped in intervals of ten. The distribution is of approximately normal type, there being a slight negative skewness −0·17. This comparative lack of skewness can be attributed to the lack of skewness in the answer-pattern-differential. On the other hand, no feature of the answer-pattern-differential can account for the normal or Gaussian nature of the score-scatter. This normality is a property of the population tested and not of the test used.

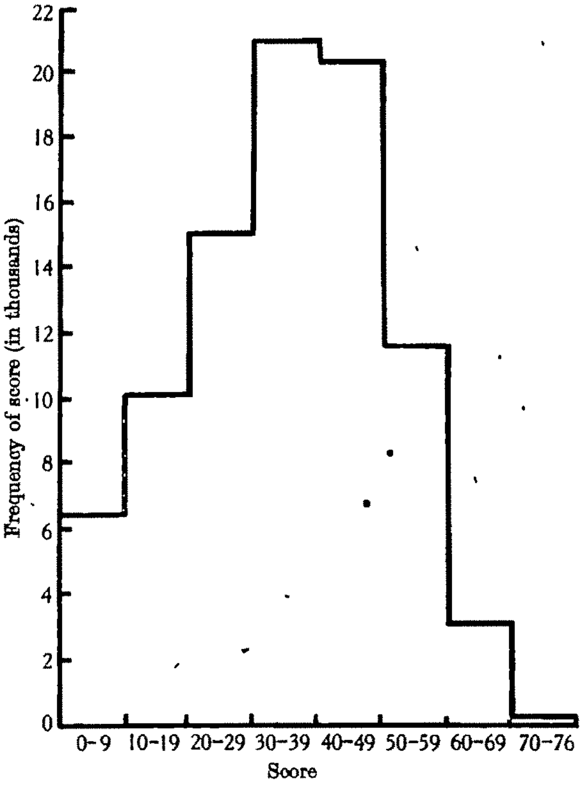

Fig. 2. Score-scatter of verbal test.

(2) *The pictorial test*

This test consisted of nine items, each carrying unit score. It was the sole survivor of three tests of the pictorial type, the other two having to be eliminated for various reasons. The answer-pattern was obtained from the papers of 450 boys and 450 girls selected at random as before. Here again the random nature of the selection was found to be satisfactory, and the application of Pearson's test of Goodness of Fit showed that the differences between the two patterns could be attributed to the effects of sampling. The combined answer-pattern is shown in Fig. 3; the figures from which it is obtained are included in Table I.

It will be observed that the test was a very easy one; it was, in fact, intended to give the weaker candidates scope. The answer-pattern is flatter than that of the verbal test, and produces an answer-pattern-differential with a tail to the left and a hump to the right, as shown in Table I. Such a distribution has a strong negative skewness which in this case is $-1{\cdot}20$.

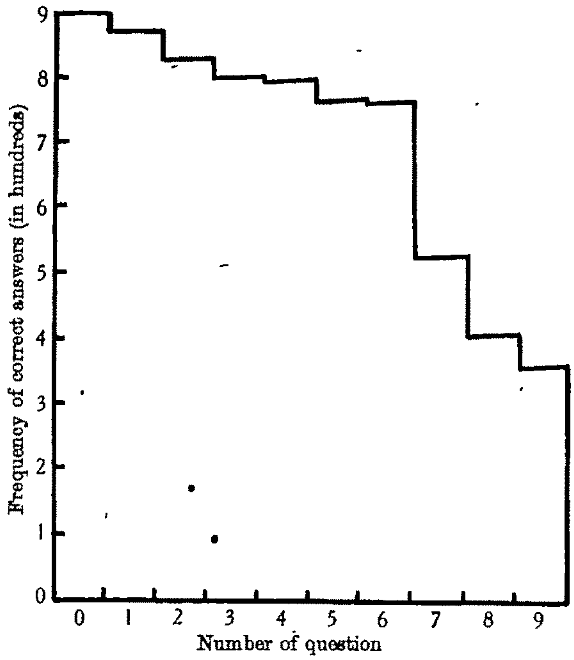

Fig. 3. Answer-pattern of pictorial test.

Table I

Item	0	1	2	3	4	5	6	7	8	9
Frequency of correct answers	900	868	829	791	761	800	764	363	519	403
Answer-pattern	900	868	829	800	791	764	761	519	403	363
Answer-pattern-differential	32	39	29	9	27	3	242	116	40	363
Score-scatter (in hundreds)	16	16	21	27	30	52	135	186	205	187

The score-scatter obtained from the papers of all 87,498 candidates has a coefficient of skewness $-1{\cdot}33$. Its histogram is shown in Fig. 4 and the frequencies to the nearest hundred are given in Table I. Now this strongly skewed score-scatter was obtained from the group of candidates which yielded the normal score-scatter of Fig. 2. The sole difference has been in the test used, and, as predicted, the strong negative skewness of the answer-pattern-differential has produced an equally great negative skewness in the score-scatter.

In Table I the figures of the answer-pattern-differential are not strictly comparable with those of the score-scatter, since their totals are 900 and 875 respectively, but the obvious resemblance of the two sets is little affected by this slight difference.

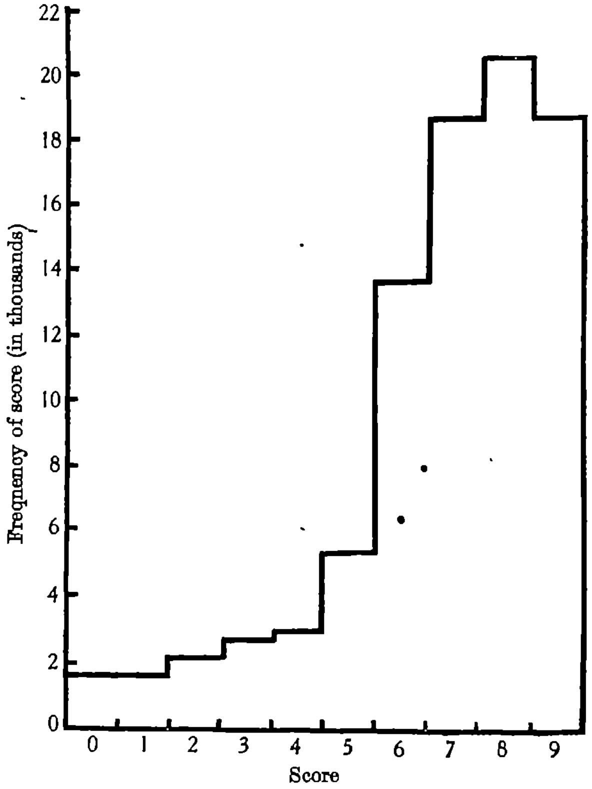

Fig. 4. Score-scatter of pictorial test.

The results of these tests show the reality of the connexion between answer-pattern and score-scatter. The connexion is not only real but useful. Let us suppose that in the try-out of a test such as the one considered there was obtained a score-scatter of a type not desired by the examiner. Then the task of adjusting the test-paper is changed by the application of this theory from a trial and error basis to the methodical adjustment of the answer-pattern to match the score-scatter desired.

III. THE RELATION OF DIFFICULTY LEVEL AND SKEWNESS OF ANSWER-PATTERN-DIFFERENTIAL

The strength of the relation between the answer-pattern-differential and the score-scatter of a test may be measured in one aspect by correlating skewness of answer-pattern-differential with skewness of score-scatter over a number of tests. Such an experiment was reported in a previous paper (2) and the results showed the correlation to be of the order 0·6–0·8. It was pointed out in the same paper that a third variable, the difficulty level of the test, was also highly correlated with skewness of score-scatter, and, to make matters still more complicated, was highly correlated with skewness of answer-pattern-differential. For example, in one series of tests the correlation of skewness of score-scatter (S) with skewness of answer-pattern-differential (S') was 0·617; that of S with difficulty level d was 0·514; while the correlation between S' and d was 0·846. The regression equation predicting S from S' and d was

$$S = 0{\cdot}64S' - 0{\cdot}03d,$$

indicating that S' is the all-important factor in the prediction of S.

Unfortunately for the certainty of this conclusion, the difference between $r_{SS'}$ and r_{Sd} is not statistically significant. It would seem at first sight that another sample of tests might yield a value of r_{Sd} greater than $r_{SS'}$, and then the regression equation would give d as the all-important factor in predicting S. On the other hand, all but one of the sets of tests used gave values of $r_{SS'}$ greater than r_{Sd} though not significantly so. This was the uncertain position when the last paper was published.

Further work has yielded a solution to this problem, the key to which lies in the high correlation of S' and d. In measuring S' and d we are to some extent measuring the same thing. It will now be shown that the difficulty level d is an approximation to S', the skewness of the answer-pattern-differential.

S' is calculated from the answer-pattern-differential by the method of moments illustrated in the previous paper (2). If the test is flat, i.e. if all the items are equally difficult, the level being d, then calculation of S' produces, after simplification, the result $\frac{2d-1}{\sqrt{\{d(1-d)\}}}$, a function of d which we may denote by $\phi(d)$. The skewness of the answer-pattern-differential of a flat test can thus be calculated directly from its difficulty level. Now it can be shown mathematically that for *any* test, a first approximation to S' is given by $\phi(d)$. The goodness of the approximation varies with the type of the answer-pattern. The process of approximation may be carried a stage further. Since d does not usually differ greatly from 0·5 a first approximation to $\phi(d)$ is given by $4d-2$. The error introduced by the last approximation is easily calculated; for values of d between 0·3 and 0·7 it is less than 10 %. For correlation purposes d and the linear function $4d-2$ are interchangeable; thus d is an approximation to $\phi(d)$, which in turn is an approximation to S'.

Thus the well-known rule-of-thumb method of skewing score-scatters by the variation of difficulty level appears as an approximation to the basic principle, that the skewness of a score-scatter is partly controlled by the skewness of its answer-pattern-differential. Since it is an approximation this use of difficulty level cannot be expected to yield such accurate results as that using the answer-pattern-differential; this is demonstrated by the diminution in the correlations found in two series of tests, shown in Table II.

Table II

	First series	Second series
r_{SS}	0·628	0·836
$r_{S\phi}$	0·576	0·816
r_{Sd}	0·561	0·790

For this reason a better prediction of S will always be obtained from S' than from d. From a study of the whole answer-pattern one gains a better idea of the probable skewness of the score-scatter than would be obtained from a knowledge of the test's difficulty level alone.

An interesting commentary on the above chain of reasoning is provided by the regression equation connecting S' and d, calculated from the results of one series of tests. When S' and d are measured in their original units, this equation reads $S'=4{\cdot}10d-2{\cdot}07$, which compares well with the approximation $4d-2$ for S'. The absence of values of d outside the range 0·19 to 0·66 probably accounts also for the apparent anomaly that S was found to be correlated in linear fashion with such varying functions of d as d^3, $\phi(d)$ and d itself. For these tests of linearity the method employed was that given by Fisher (5), replacing Blakeman's criterion, which was used in the previous paper.

IV. THE VARIATION OF FIT OF ANSWER-PATTERN-DIFFERENTIAL AND SCORE-SCATTER

It was shown in the first paper of this series (1) that the answer-pattern-differential and score-scatter of a test are identical in the particular case referred to as 'unig', where each candidate's score is compiled of answers to the easiest questions. The experiments previously reported and the results of the Scottish Tests analysed in this paper, show that, in the general case where random answering is present, there is still some measure of relation or correspondence between the two distributions.

One measure of correspondence that has been used is the correlation coefficient of corresponding statistics (e.g. the coefficient of skewness) of the distributions. The calculation of these correlations implies the existence of several tests applied to the same or similar populations, and the results obtained apply to the group of tests as a whole. To study the variation of fit of answer-pattern-differential and score-scatter from test to test, it is necessary to devise some measure of this fit applicable to the results of a single test.

First it may be pointed out that the answer-pattern-differential and score-scatter are usually so removed from identity that no use can be made of Pearson's Test of Goodness of Fit. In any case this test can be used only to decide whether or not two distributions may be regarded as identical save for the effects of random sampling; it cannot be used to measure the degree of resemblance as the title "goodness of fit" might suggest.

(1) *Criticism of h*

In the first paper of this series (1) an account was given of a coefficient devised for this purpose. Denoted by *h*, it was termed the coefficient of 'hig', being intended to measure the poorness of fit that accompanies higgledypiggledy answering. Further research has shown that the definition of *h* is based on assumptions that involve over-simplification of the problem. The word hig was used to cover three conceptions which at that time had not been observed to be distinct. These are:

(i) randomness of answering, for which the term hig should properly be reserved;

(ii) poorness or goodness of fit of answer-pattern-differential and score-scatter, for a measure of which we are seeking;

(iii) control of score-scatter by answer-pattern-differential, i.e. the fit of the score-scatter to a score-scatter calculated from the answer-pattern-differential but not necessarily identical with it.

These concepts are linked by the fact that complete absence of random answering implies perfect fit and perfect control. But we must abandon the assumption, made in the definition of *h*, that the poor fit that accompanies complete randomness of answering, as shown by a flat answer-pattern, is due to its random nature, and that there is in this case no control of the score-scatter by the answer-pattern. Examination of the answer-patterns and score-scatters of many tests has made it clear that the poor fit accompanying completely random answering is due mainly to a property of score-scatters ignored in defining *h*, the tendency of all score-scatters to the normal or Gaussian type of distribution. Thus the good fit obtained with steep tests B and C of the first paper (1), and the poor fit found with test A there, may be accounted for by the answer-pattern-differentials of B and C being already akin to normal distributions, while that of A was far from normal. Even where poor fit is shown, as in these flatter tests, there is evident some control of score-scatter by answer-pattern-differential; occasionally even the double hump characteristic of the answer-pattern-differential of a flat test is reproduced in the score-scatter.

(2) *Attempt at a second method*

This line of argument led to the devising of a second coefficient to measure the relationship between answer-pattern-differential and score-scatter. The score-scatter obtained was regarded as the resultant of the answer-pattern-differential and a normal distribution characteristic of the population tested, and the relative strengths of these two factors were measured. Although results were obtained for one set of tests, the method is not generally useful as the theoretical basis for the characteristic normal distribution is insufficient for most tests. Other coefficients were devised, tested, and rejected for similar reasons.

(3) *Indirect method using the idea of steepness*

It would seem then that failure must be recorded in our attempts to measure that correspondence which can often be observed in the results of a test. There is, however, one other method of attack; though it does not yield a solution of the problem the results seem to dispose of it in another way by suggesting that in general it does not arise; that in the tests in common use the fit or control we seek to measure varies only slightly. For this avenue of attack we return to the idea of steepness.

It was proved in an earlier paper (1) that the probability of unig (which implies perfect fit of answer-pattern-differential and score-scatter) was greater with steep tests than with flat tests. If we have failed to differentiate tests directly by the degree of correspondence between their answer-pattern-differentials and score-scatters, we may attempt to do so indirectly by measuring their steepness.

The definition of steepness used in the proof mentioned above is merely the statement that steepness implies large differences in the difficulties of the items, as measured by the numbers of correct answers. This implies (i) that the number of items is small; (ii) that these items make the greatest use of the available difficulty range. The second implication will be considered first. One way to make use of the whole difficulty range is to make the items progress uniformly in difficulty from extreme ease to extreme difficulty, so that the graph of the answer-pattern is a straight line. The curve which makes the probability of unig a maximum is not strictly a straight line, but the whole calculation is of such mathematical difficulty, and the result so far as obtained so nearly a straight line, that this has been adopted. Now a straight line is the shortest distance between two points. Any other answer-pattern including the initial value n_0, the number of candidates, and falling to zero after the last item, has therefore a longer path, the length of which is a measure of the departure from steepness. On this reasoning there was based a coefficient of steepness which could be used to compare the steepnesses of tests which had the same number of items. Each of the forty-one tests previously referred to had ten items, and by the use of this measure of steepness they were divided into two groups of steeper and flatter tests, the dividing line being fixed to place approximately equal numbers of tests in each group. Each group then provided data from which could be calculated the correlation of corresponding statistics by the method used in the previous paper (2).

When this was done the results were as follows: The correlation of the standard deviations of answer-pattern-differential and score-scatter for the twenty steeper tests was 0·78; for the twenty-one flatter tests it was 0·83. The difference is not statistically significant. In the matter of standard deviation the flatter tests show a fit which is better, but not significantly so. The correlation of the skewness of the answer-pattern-differential with the skewness of the score-scatter for the twenty steeper tests was 0·80; for the twenty-one flatter tests it was 0·59. Again the difference is not statistically significant. Here the better fit is obtained from the steeper tests. From the scanty data at our disposal it would seem then that the control of score-scatter by answer-pattern-differential is no greater in the case of the steeper group than in the case of the flatter group.

This conclusion, which at best is only tentative, is supported by the result obtained in a second experiment with the results of the forty-one tests. The steepness coefficient was correlated with the coefficient already mentioned measuring the relative influence of the answer-pattern-differential and a hypothetical normal distribution. The correlation was found to be 0·218, which though positive is not significantly different from zero, being derived from forty-one pairs of values. The steeper tests do not show a significantly greater degree of control by the answer-pattern-differential.

(4) *Conclusions*

It would appear then that for tests of ten items or more, the control of score-scatter by answer-pattern-differential is practically independent of the steepness of the test. This is rather a welcome conclusion since it simplifies matters somewhat. To produce certain types of score-scatter it is necessary to use answer-patterns of the flatter type. If the certainty of control decreased as the pattern became flatter, the position would be much more confused than it is. As far as these results go, they show that the flatter answer-pattern exerts about as much control over the score-scatter as does any other type.

These conclusions are for tests having ten items or more. If the test has very few items the position is altered. In such a test the differences in difficulty between the items can be made large, so that the test is really steep in the sense that the probability of unig is high. In any ten-item test the probability of unig must always be so low as to be negligible, however well the items use the range of difficulty. To produce probabilities of the order of 0·2 the test must have no more than four items, while probabilities of the order of 0·6 restrict the number of items to three. But this is a type of test few examiners would care to use; it would very easily give rise to difficulties through misunderstandings, prior knowledge, and guesswork, all of which tend to be smoothed out in a test with many items.

We are thus in the position of having gained upon the roundabouts what we lost upon the swings. We have lost hope of constructing useful unig tests, but have gained a fair degree of certainty that the flatter tests show as great a correspondence between answer-pattern and score-scatter as do the steeper tests.

V. SUMMARY

1. The score-scatters and answer-patterns obtained from the tests used in the Scottish Mental Survey show on examination that relation which has previously been predicted and illustrated by smaller scale experiments. A particular point of interest is the production by the same population of score-scatters differing widely in skewness under the influence of appropriate answer-patterns.

2. The rule that a difficult test tends to produce a positively skewed score-scatter is shown to be a particular case of the general principle that

skewness of score-scatter is highly correlated with skewness of answer-pattern-differential.

3. As far as can be ascertained from the available data, the tie between answer-pattern and score-scatter is equally strong for steep tests and for tests that tend to the flat type.

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PUBLICATIONS RECENTLY RECEIVED

Theories of Sensation. By A. F. Rawdon-Smith. Cambridge University Press. 1938. Pp. xiii+137. 10*s.* 6*d.* net. New York: Macmillan. $2.75.

It is not easy for psychologists to keep abreast of the rapid advances which are being made in the physiology of the special senses and central nervous system. Dr Rawdon-Smith's book will therefore be of very great value for its surveys of the most important work, and especially the recent investigations, on vision and hearing. The author possesses a remarkable talent for summarizing intricate theories and experiments both concisely and clearly. His treatment will be too advanced for the majority of students, but will be extremely useful to those who are already familiar with the ordinary information about the special senses contained in standard psychological textbooks.

If one has a complaint to make, it is on the ground that the book is essentially a physiological, not a psychological, monograph. As the title indicates, it discusses visual and auditory sensations, without any regard for the psychologist's doubts and difficulties as to the reality of sensations. For example, there is no mention of Katz's work in the detailed account of colour vision, nor any indication that the 'bundle' and 'constancy' hypotheses of nerve conduction and cortical activity are suspect. The physiologist is apparently as atomistic in his approach as ever he was. This criticism does not, of course, reduce the value of the brilliant experimental work of recent years which Dr Rawdon-Smith describes (and in which he has himself played no small part). But it does mean that such work tells us scarcely anything fresh about the psychology of visual or auditory perception. And it is unfortunate that the gap between physiology and psychology should, apparently, be widening rather than decreasing.

The book is excellently produced and printed. The cost would seem rather excessive, were it not that the book contains so large a quantity of highly concentrated information. It is a pity that the bibliography is only a skeleton one. Both the titles and the full page references of books and articles would often be useful to the reader.

P. E. V.

Frustration and Aggression. By John Dollard, Leonard W. Doob, Neal E. Miller, O. H. Mowrer and Robert R. Sears. New Haven: Yale University Press (London: Humphrey Milford). 1939. Pp. ix +209. 9*s.* net.

It is somewhat difficult to understand the aim of this book. The authors begin with the postulate that "aggression is always a consequence of frustration" and, moreover, that frustration always leads to some form of aggression. There are four supplementary principles: the amount of aggression varies with the amount of frustration; the strength of inhibition of an act of aggression varies positively with the amount of punishment to be anticipated as a consequence of that act; inhibited aggression becomes displaced or altered, either as regards its object or as regards its form; the occurrence of an act of aggression will reduce the 'instigation' to subsequent aggression. All these tenets are illustrated by numerous examples drawn from everyday life, particularly the development of children, from criminology, sociology and ethnology. So far so good; there are few people who would deny that acts of aggression frequently result from frustration and occur in accordance with the principles described above. Moreover, it is most interesting and valuable to have the various instances arranged and systematized within the same scheme. But where do we go from here? Since we learn nothing about the nature of frustration and aggression, their psychological as apart from their environmental causation, we cannot tell why certain of the above principles operate in some instances, others in others. Nor could we account for the numerous instances in which neither the above principles, nor the original basic hypotheses, appear to operate. The truth is that, in the absence of some causal hypothesis, however tentative, the average reader is at a loss as to how to schematize his knowledge. And the psychologist must go on adding to his pile of facts about behaviour, not knowing how to distinguish between the significant and the trivial. The authors acknowledge their debt to Sigmund Freud; would that it had been greater!

Technique of Analytical Psychotherapy. By WILHELM STEKEL. Translated from *Technik der analytischen Psychotherapie*, by EDEN and CEDAR PAUL. London: John Lane, The Bodley Head. 1939. Pp. xvii + 408. 21*s*. net.

Although this book is intended primarily for those who practise, or hope to practise, psychotherapy, it gives a plain and comprehensive description of the practice of psycho-analysis which will be of interest and value to any psychologist. The latter will be particularly glad to be relieved of the mythological hocus-pocus which emanates from so much of the writing of the Freudian psycho-analysts. Not that Dr Stekel in any way underrates the paramount importance of Freud's contribution to psychological knowledge. But he does speak out strongly against some of the Freudian shibboleths: the stressing of infantile sexual traumata and of the castration complex in neurotic conflict; the necessity of apparent passivity on the part of the analyst; and the advisability of very lengthy and exhaustive analyses. Though convinced of the importance of intuitive interpretation and of active intervention in the course of the analysis (and thereby stressing the importance of the analyst's own character and of his awareness of his own 'blind spots'), Dr Stekel roundly condemns the atmosphere of esoteric mystery and the slavish acceptance of authority and dogma which have crept up among many Freudian analysts. But is it possible that the attribution of every form of neurotic conflict to a sexual origin is a 'blind spot' of Dr Stekel's?

The author's desire not to be dogmatic, to qualify and illustrate all his conclusions has, however, rendered his presentation somewhat prolix and discursive; and the reader may at times find it hard to follow the thread of his argument. The book would be improved by pruning. There is a really good chapter on the Psychoanalysis of Obsessional Disease, previously read as a separate paper, and presented more logically and systematically than the remainder. But the reader who perseveres will be rewarded by a real understanding of the practice of psychoanalysis, its dangers, its difficulties and its manifest value.

The Language of the Dream. By EMIL A. GUTHEIL. New York: The Macmillan Company. 1939. Pp. 286.

The author of this book is an associate and disciple of Stekel, and he gives a full, clear and able account of dream analysis by the 'intuitive', 'active' and 'economical' technique employed by Stekel. In this, little, if any, use is made of the patient's free associations to the dream content, although analysis is of course made in the light of the particular circumstances of the case. The book gives detailed descriptions of dream mechanisms and symbolisms and of the analytical interpretations of many dreams of normal, neurotic and psychotic cases. Finally, a short but illuminating chapter contrasts some interpretations made by Stekel and his followers, with the corresponding interpretations made by Freud, Adler and Jung. The reader is left with the uneasy feeling that the interpretation is mainly a function of the analyst's preconceived opinions and theories. Stekel's interpretations are, in general, the least fantastic, and his method has the great advantage of making psychotherapeutic treatment more rapid. There is no doubt that in his hands dream analysis is an efficient tool. But this does not substantiate the ultimate validity of his readings of the language of the dream.

New Ways in Psychoanalysis. By KAREN HORNEY. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1939. Pp. 313. 12*s.* 6*d.* net.

In the light of her experience as a psychoanalyst and of the doubts which it se t up as to the full validity of Freudian psychoanalytic theory, Dr Karen Horney sets out to make a critical revaluation of that theory, and to eliminate its more erroneous features. She condemns particularly Freud's adherence to instinctivist and genetic explanations and derivations, and to his insistence on a purely biological determination, e.g. by anatomical sex differences as in the castration complex. Dr Horney, on the contrary, considers that "the relevant factor in the genesis of neuroses is neither the Oedipus complex nor any kind of infantile pleasure strivings, but all those adverse influences which make the child feel helpless and defenceless....Narcissistic, masochistic, perfectionistic trends...are not derivatives of instinctual forces, but represent primarily an individual's attempt to find paths through a wilderness full of unknown dangers. The manifest anxiety in neuroses is not the expression of the 'ego's' fear of being overwhelmed by the onslaught of instinctual drives or of being punished by a hypothetical 'super-ego', but is the result of the specific safety devices' failure to operate."

This thesis Dr Horney develops most ably and persuasively. But her presentation is somewhat marred by her failure to cite any detailed case histories; she appears to select from them such isolated features as will best prove her case. This omission may be necessary in part in order to save space. But the reader, interested and impressed though he undoubtedly will be by the inherent psychological probability of her argument, would like to study in more detail the evidence upon which it is based.

Play Therapy in Childhood. By C. H. ROGERSON. Oxford Medical Publications. Oxford University Press (London: Humphrey Milford). 1939. Pp. 64. 3*s.* 6*d.* net.

This brief monograph describes work carried out mainly in the Department of Psychological Medicine at Guy's Hospital on the effects of play therapy on certain rather difficult cases of neurotic disturbance in children. The effect of the treatment was apparently beneficial; but it is not possible to say how far this was due to the actual 'play' treatment, and how far to the friendly relations established between the child and the therapist. The reports of cases and the discussion are too slight to add much to our knowledge of the function of play in childhood.

The Intelligence of a Representative Group of Scottish Children. By A. M. MACMEEKEN. London: University of London Press, Ltd. 1939. Pp. xvi + 144. 5*s.* net.

When the survey of the intelligence of a whole age-group, described in *The Intelligence of Scottish Children,* was carried out, at the same time a sample of 1000 children from the group was given the 1916 Stanford Revision of the Binet tests. But this smaller group did not form a random sample. Therefore, it was decided by the

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Scottish Council for Research in Education to test a further group of about the same size, but selected from all areas and all social classes in Scotland, with the Stanford Revision and also with a Performance test scale. In addition, all the Binet tests and most of the Performance tests were performed by a single investigator, Miss MacMeeken, to avoid variations in assessment between different investigators. In this way a set of very valuable data was obtained; and this book gives a lucid account of some of the results of this most interesting and important experiment. The distributions of (Binet) intelligence and of Performance test scores are discussed, and are compared as between boys and girls, and as between urban and rural areas. Further results will appear in due course.

Biographies of Child Development. By ARNOLD GESELL, CATHERINE S. AMATRUDA, BURTON M. CASTNER and HELEN THOMPSON. London: Hamish Hamilton, Ltd. 1939. Pp. xvi+328. 15*s*. net.

This book contains in its first part follow-up studies of typical cases described in Dr Gesell's previous publications; and in its second part, individual case studies illustrating certain peculiarities in growth and development. But all these studies are intended to show that, by using Dr Gesell's technique of assessing the degree of development of certain main characteristics, it is possible in infancy and early childhood to predict with considerable accuracy what later development will occur in the adolescent and adult. Since physical, intellectual and motor development are comparatively easy to measure, it is these which can be most accurately predicted. Social development can also be described, but the development of emotional characteristics and of the personality is shown only vaguely. In consequence, the reader is left with the impression of separate lines of development of isolated characteristics which never become synthesized into a completed personality. Useful as Dr Gesell's technique undoubtedly is in predicting the future development of the more obviously apparent characteristics, it can as yet tell us little beyond the use of general terms such as 'stable', 'immature', 'excitable', etc., about the kind of 'person' the child will become.

The type face employed is too 'arty' and unsuitable for printing this kind of book, and, moreover, interferes distinctly with its legibility, at least for some readers.

Child Psychology. By FOWLER D. BROOKS. London: Methuen and Co., Ltd. 1939. Pp. xxx+600. 12*s*. 6*d*. net.

This book provides an admirable compendium of the vast and growing American literature on Child Psychology. It only touches on the work of the Austrian school, and omits all reference to that of the Gestalt psychologists and of English child psychologists. Since the American literature is mainly behaviouristic in outlook, the child's overt behaviour and physical characteristics are dealt with more adequately than are its less obvious psychological characteristics. Motivation, emotion and personality are discussed rather superficially, with insufficient reference to underlying basic structures and mechanisms.

Within its limitations, the book is a good one of its kind, and should be useful to the specialist in child psychology as a source of reference. But it would be a mistake to let it add more weariness to the flesh of the English psychological student. And it is not up to the standard of Messrs Methuen's "Contributions to Modern Education" series. The introductory 'puff' by an un-named editor is unnecessary.

Nursery School Education and the Reorganization of the Infant School. By OLIVE A. WHEELER and IRENE G. EARL. London: University of London Press, Ltd. 1939. Pp. 172. 5*s*. net.

At a time when schools are being disorganized and educational reform retarded, and when parents find it increasingly difficult to spend much time with their children, it is most fitting that the case for the Nursery School should be pressed; and that every attempt should be made to substitute it for the traditional Infant School. The Nursery School not only relieves the harassed mother during the daytime of the care of little children aged 3 to 6, but also gives them opportunities of free and healthy living which they would otherwise miss, and helps them to acquire good habits of eating, sleeping, cleanliness and social co-operation.

In this inexpensive and charmingly illustrated book, Prof. Olive Wheeler describes the psychological characteristics of the child and shows how they are developed and advantaged by the Nursery School. Miss Earl describes in more detail the school activities. Though there is nothing novel in the book, it is clear and readable, and should provide excellent propaganda for the spread of Nursery Schools.

Religion and the Growing Mind. By BASIL A. YEAXLEE. London: Nisbet and Co., Ltd. 1939. Pp. x+224. 7*s*. 6*d*. net.

The first chapter of this work is largely polemical. Its attempt is to show that those modern psychological theories which seem to imply that religious impulses and experience have a perfectly naturalistic explanation are unacceptable. As would be expected, the main arguments are directed against Freudian and allied interpretations. In subsequent chapters general mental development from early years through adolescence to maturity is followed and for each stage of development the author tries to indicate clearly the characteristic appeal and forms of religion. The final parts of the book endeavour to apply all the conclusions reached in a practical way to problems of the home, the school and the world. The author has read widely and makes great use, sometimes in a rather scrappy fashion, of a large range of authorities. The book is sincere, honest and fair; but it is not very original. It has no difficulty in showing how extremely deep-rooted in man are religious yearnings and beliefs. But the critical reader will realize that when it passes to statements of a specific kind of reality answering to those yearnings and beliefs, he is as much beyond psychology as are his opponents who assert that there is no such reality. In fairness to the author it should be said that he is himself more frankly aware of this than many of those who have asserted the illusory character of religion, basing their assertion on psychological grounds, have often appeared to be.

Fit after Forty. By CLEMENT JEFFREY. London: Frederick Muller, Ltd. 1939. Pp. 213. 5*s*. net.

This is a book of good advice. It need not have the effect of making people worried, for it is sensible enough, and in no way extreme or frightening. The topics discussed are: Diet, Constipation, Getting through the Winter, the Change of Life and Mind after Forty. The final chapter is the only specifically psychological one, and at any rate it is some recommendation to say that it does not contain anything that will greatly surprise the psychologist. It is rather platitudinous, but there are always plenty of people to whom platitudes come for the first time and who may benefit greatly from them.

Nouveau Traité de Psychologie. Par GEORGES DUMAS. Tome VI. Paris: Félix Alcan. 1939. Pp. iv + 544. 125 fr.

The sixth volume of this very fine 'new treatise' deals with the systematic functions of the affective and active behaviour of human beings. Prof. Dumas has been both skilful and fortunate in his choice of collaborators, and the result is a volume which more than maintains the high standard of its predecessors. The topics and authors represented in this book are: *La Logique des Sentiments* and *Les Passions*, by L. Dugas; *l'Évolution, la Spiritualisation et la Socialisation des Tendances*, by F. Challaye; *l'Amour et la Haine*, by D. Lagache; *les Sentiments Sociaux et les Sentiments Moraux*, by C. Davy; *les Sentiments Religieux*, by C. Belot; *les Sentiments Esthétiques et l'Art* and *l'Invention et le Génie*, by H. Delacroix; *les Volitions*, by Ch. Blondel; and *Psychophysiologie de l'Effort Physique*, by H. Laugier and W. Liberson. It will be seen that all of the essays, with the exception of that dealing with the psycho-physiology of muscular work—and this, by the way is an extremly good and well-documented survey of experimental work—call rather for delicate powers of psychological analysis and a wide knowledge of human life than for a strict laboratory procedure. And anybody who is acquainted with the brilliant stream of French contributions to the study of feeling and emotion from Ribot onwards, will approach this book with high hopes. Nor is he likely to be disappointed. For the essays are, for the most part, delightful to read, full of literary as well as of scientific allusions, and themselves artistic as well as penetrating. It is much to be hoped that this volume will be widely read and fully appreciated in this country as well as in France.

La Psychologie Expérimentale en Italie, École de Milan. Par A. MANOIL. Paris: Félix Alcan. 1938. Pp. viii + 489 + 16 plates. 80 fr.

As most English readers well know, Prof. A. Gemelli, at the University of Milan, has for a good many years been conducting important researches in experimental psychology. This book gives a very excellent account of the main groups of these investigations, with all the necessary references to the original publications of Gemelli and his associates. The studies dealt with are: emotional phenomena, researches into psychophysical methods and problems, comments on the 'thinking' horses of Elberfeld, contributions to the study of perception, the electro-acoustical study of language, the selection of air-pilots, and general industrial applications. At the conclusion of the volume the general point of view of the Milan school is briefly and clearly indicated. For those who cannot read or obtain the original Italian publications this book will be of the greatest value, for it is reliable and authoritative. There is a very good bibliography.

L'Automatisme dans l'Art: Idée et Mouvement. Par ANTOINE GIRAUD. Paris: Librairie Marcel Rivière. 1939. Pp. 127. 12 fr.

Dr Giraud may be right or wrong, but the essay which he has produced is full of interest in either case. The distinction which we commonly make between certain processes which are consciously controlled and others which spring from the unconscious is, he considers, a misleading one. There is but one source of all products of the mental life, and this is the totality of ideas which come, how it is hard and perhaps impossible to say, to the producer. Moreover, however he may appear to work, what he produces is the expression of the laws of interrelation of these ideas among themselves and in relation to him. Ideas having accumulated reach a certain state of tension which may be lesser or greater according to the amount of control that is being exercised, but when they break through the control then comes the moment or the time of expression.

Since intoxications of various kinds may weaken or release control it often happens that intoxication is the immediate occasion for the production of imaginative work. All this is clearly set forth and illustrated by many quotations from people of genius in all fields of art. A final and rather long chapter, endeavouring to show that ideas and movement are at bottom identical, so that, for example, if the former are let alone to have free play, a form of active expression must result, is not as convincing or as satisfactory as the rest of the essay. But the book is worth reading, and its most apposite quotations will be of great value to anybody who is on the look-out for any material with which to illustrate any study of imaginative inspiration.

L'Épithymologie (La Désirologie). Par JEAN URBAN. Paris: Félix Alcan. 1939. Pp. 359. 50 fr.

Epithymology is, according to the author, that branch of social psychological science which endeavours to lay bare the static and dynamic laws regulating the interrelations of desires, the connexion of desires with the varying internal and external environments and the phenomena arising therefrom. Also, from a practical point of view epithymology endeavours so to apply the laws as to achieve a free and fully voluntary existence. The volume deals first with feeling and desire, secondly with the laws of desires, and thirdly with will and the laws of conscious activity. As the title indicates, the treatment is systematic, logical, analytic, rather than experimental or even empirical. It is a book for the psychologist to read critically. If he treats it, not as a definitive treatise, but as an introduction to a very large number of little investigated problems, he will be able to gather a great many ideas and suggestions which could be submitted to more factual and immediate test.

La Mémoire et la Vie: Essai de Défense du Mécanisme Psychologique. Par E. AUGIER. Paris: Félix Alcan. 1939. Pp. 251. 30 fr.

It is perhaps a little curious that while this book is a sustained defence of strictly empirical methods in psychology, and an attack upon all contemporary tendencies to take refuge in rather vague and qualitative principles of explanation, it has itself very little to do with directly recorded data and moves almost wholly in the realm of concepts. Dr Augier argues that precisely the same general framework of ideas that are used in physics, chemistry and the other natural sciences apply in just the same ways to all psychological problems. This he considers to be most evident in regard to the study of memory which he pursues, in a very general manner, first as he finds it illustrated in inorganic matter, then in general biology, then in behaviour and finally in human psychology. Memory he considers always to have two forms: "inscription des traces du passé, évocation de ces traces sous forme de souvenir," and the two together "forme une base explicative compléte et commode pour tous les faits psychologiques". It is a pity that this view, which is certainly less uncommon than the author seems to believe, is argued in a somewhat controversial manner rather than demonstrated. Nevertheless, the essay contains much to interest any psychologically trained reader, and perhaps later Dr Augier may put his general arguments and proposals into a more concrete and experimentally illustrated form.

Principes d'une Psychologie des Tendances. Par A. BURLOUD. Paris: Félix Alcan. 1938. Pp. 430. 60 fr.

Here is a most interesting book which deserves to attract the attention of many English readers. M. Burloud has set before himself the task of developing a complete and fundamental point of view for the study of psychology. His main affiliations are with Maine de Biran and with the development of a standpoint concerned less with the content of mental processes than with a sustained attempt to work out the characteristics, interrelations, forms of operation and conditions of these processes. At all possible points he is willing to call in the aid of experimental study, and many of the interesting experiments reported are original. Perhaps the most interesting parts of his study are those concerned with schemata, their development and their functions. He covers a very wide field, and deals not only with all the main forms of general mental activity, but with the specific evolution of active directed processes in sensation, in behaviour, in perception, in recall and in thinking. Whether or no his views about the final character of psychological explanation are accepted, his discussions and his illustrative material will be found stimulating by any well-informed psychological reader.

Conscience et Mouvement: Étude sur la Philosophie Française de Condillac à Bergson. Par GABRIEL MADINIER. Paris: Félix Alcan. 1938. Pp. 481. 65 fr.

Anybody who is interested in the use made by the outstanding French philosophical and psychological writers of the notions of activity and of dynamic ideas will find an excellent critical account in this book. The general development of these ideas is traced from Condillac to Bergson, and, from the psychological point of view, some especially interesting remarks are made about Fouillée and Renouvier. Naturally the contributions of Maine de Biran receive a great amount of attention.

Conscience et Amour: Essai sur le 'nous'. Par GABRIEL MADINIER. Paris: Félix Alcan. 1938. Pp. 143. 25 fr.

This is a philosophical essay. In a critical sense it is directed against objectifying and analysing tendencies which may lose sight of reality in a mass of dividing and individualizing distinctions. In a constructive sense it is an argument for an active attitude in which the 'I' is both realized and transcended in the 'we'. For this active attitude in its widest expression, the author finds the best name to be love.

Montaigne et l'Amitié. Par MAURICE RIVELINE. Paris: Félix Alcan. 1939. Pp. 268. 40 fr.

For the psychologically-minded reader the interest of this book lies chiefly in its excellent and appreciative study of the long and deep friendship between Montaigne and La Boétie. The essay falls into three parts, of which the first deals with friendship in the life of Montaigne, the second with the notion of friendship as held and developed by Montaigne, and the third with the influence of friendship in a general sense on the Essays. The whole is not merely an attractive excursion into literary criticism, but a significant study of one of the most important and influential of the human sentiments.

Essai sur les Origines Intuitives du Positivisme. Par PIERRE DUCASSÉ. Paris: Félix Alcan. 1939. Pp. 272+4 plates.

To many people positivism appeared, and, in so far as it is still influential, continues to appear, as a way of escape from a subjective and meditative way of thinking to something more externally directed and more in touch with the actual movement of life. At the same time it seemed to many to be rather a 'knack' of a systematic way of thinking, sweeping, regular, uniform, and not over-accommodating to the constant change of affairs. So Renan, for instance, made his famous prophecy, quoted near the beginning of this book: "I've come to believe that M. Comte will be 'une etiquette' in the future. He will have an important place in the future histories of philosophy. This will be a mistake, I'm sure, but the future will make lots of other mistakes!" What M. Ducassé is largely concerned to show, is that positivism cannot be understood simply as an attempt to build a scientific philosophy. Its basis lies in Comte's own intuitive and affective life. Hence this essay is to a degree an intellectual biography of a philosopher. It is an extremely readable and lively essay, though to the English reader of the present day it may well seem a little on the long side.

The Boundaries of Science. By JOHN MACMURRAY. London: Faber and Faber, Ltd. 1939. Pp. 268. 7*s.* 6*d.* net.

A Bibliography of Psychological and Experimental Aesthetics, 1864–1937. By ALBERT R. CHANDLER and EDWARD N. BARNHART. University of California Press (England: Cambridge University Press). 1938. Pp. 189. 9*s.* net.

The Group Mind. By W. MCDOUGALL. Cambridge University Press. 1939. Pp. 304. 7*s.* 6*d.* net. A cheap edition of McDougall's well-known treatise.

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BRITISH PSYCHOLOGICAL SOCIETY

REVENUE ACCOUNT AND BALANCE SHEET AS AT 30 SEPTEMBER 1939

Revenue Account for the year ended 30 September 1939

1938 £ s. d.		EXPENDITURE	£ s. d.	£ s. d.
170 0 0		*To* Rent, 55 Russell Square		180 0 0
252 0 0		Secretary's Salary		252 0 0
		Part-time assistance in Office ...		16 0 9
93 5 0		Printing and Duplicating		109 16 7
25 15 6		Expenses of Meetings		25 11 0
		Office and General Expenses:		
	£33 6 5	Caretaker, Lighting and Heating	33 11 8	
	15 12 1	Stationery	18 6 0	
	68 0 9	Postages and Telephone ...	79 8 8	
	4 12 0	Fire, etc. Insurance	4 12 0	
	4 14 6	Subscriptions to outside Associations	4 14 6	
	10 10 0	Audit Fee 1938	10 10 0	
	7 13 11	*Repairs*		
	9 15 3	Teas	14 15 6	
	9 8 6	Sundry Expenses	5 11 5	
163 13 5				171 9 9
		Journals Publication Expenses:		
	263 19 6	General	234 2 7	
	124 14 9	Medical	130 6 6	
	100 0 10	Educational	98 12 6	
488 15 1				463 1 7
11 16 8		Extended General Meeting 1939 ...		9 10 7
19 1		Work for Committee on Human Mental Measurements... ...		1 15 11
4 4 0		Midland Branch Expenses... ...		2 11 8
3 4 6		Northern Branch Expenses ...		1 5 0
11 0 5		Scottish Branch Expenses... ...		8 12 5
1 0 2		Cheque Books		1 0 0
		Depreciation:		
		Library	61 17 9	
83 3 4		Office Furniture and Fittings ...	17 13 0	79 10 9
1308 17 2				1322 6 0
140 11 0		Balance, being Surplus of Income over Expenditure		176 13 10
£1449 8 2				£1498 19 10

1938 £ s. d.	INCOME	£ s. d.	£ s. d.
	By Subscriptions 1939	1475 0 6	
	Subscriptions 1938	4 5 6	
		1479 6 0	
1325 10 6	*Less* Resignations and Removals ...	107 8 6	1371 17 6
123 17 8	Interest on Investments		127 2 4
£1449 8 2			£1498 19 10

Balance Sheet as at 30 September 1939

1938 £ s. d.	LIABILITIES	s. d.	£ s. d.
8 2 6	Subscriptions paid in Advance ...		2 2 0
	Publications Reserve:		
122 0 0	Balance as per last Balance Sheet ...		122 0 0
	Revenue Account		
	Balance as per last Balance Sheet ...	4906 5 1	
4906 5 1	*Add* Surplus of year	176 13 10	5082 18 11
£5030 7 7			£5207 0 11

1938 £ s. d.	ASSETS	£ s. d.	£ s. d.
210 2 1	Cash at Barclays Bank Ltd.		274 15 7
16 6	Postage Stamps in hand		1 19 1
	Investments at Cost:		
	£300 5% Conversion Loan 1944–64...	318 9 9	
3743 17 6	£3820. 14*s.* 8*d.* 3½% Inscribed War Loan	3425 7 9	3743 17 6
	Note. Market value of above Securities at 30 September 1939, £3251		
	Sundry Debtors		27 13 4
129 15 0	Subscriptions in Arrear		183 0 0
	Office Furniture and Fittings		
	Balance as per last Balance Sheet ...	172 3 0	
	Additions during year	3 10 0	
		175 13 0	
172 3 0	*Less* Depreciation	17 13 0	158 0 0
	Library		
	Balance as per last Balance Sheet ...	576 5 0	
	Add Expenditure during year ...	43 2 9	
		619 7 9	
576 5 0	*Less* Depreciation	61 17 9	557 10 0
	Monograph Publications:		
	Balance as per last Balance Sheet ...	147 8 6	
	Add Grants made during year ...	90 0 0	
		237 8 6	
147 8 6	*Less* Amounts repaid during year ...	27 3 1	210 5 5
50 0 0	Shares in the British Journal of Educational Psychology Ltd., at Cost ...		50 0 0
£5030 7 7			£5207 0 11

I beg to report that I have examined the foregoing Accounts with the Books and vouchers of the British Psychological Society and that I have obtained all the information and explanations I have required. In my opinion the Balance Sheet is properly drawn up so as to exhibit a true and correct view of the state of the Society's affairs according to the best of my information and the explanations given to me and as shewn by the Books of the Society. The Securities appearing under the heading Investments have been verified by production of the Bankers' Certificate.

T. BARRETT,
Incorporated Accountant

S. J. F. PHILPOTT,
Hon. Treasurer

31 *October* 1939.

VOLUME XXX APRIL, 1940 PART 4

THE RELATION OF COGNITION AND PHANTASY IN CHILDREN.

BY M. D. VERNON

From the Cambridge Psychological Laboratory

PART I

I. *Introduction* (pp. 273–275).
II. *Method of experiment* (pp. 275–278).
III. *Normal cognition and phantasy* (pp. 278–294).
    (1) *Perception and immediate recall of meaningful pictures* (pp. 278–283).
    (2) *Perception of meaningless designs* (pp. 283–287).
    (3) *Free construction* (pp. 288–294).
References (p. 294).

I. INTRODUCTION

IT has been shown by Ruth Griffiths, in her book *Imagination in Early Childhood* (5), that the little child, of five years or thereabouts, is not able to think coherently and logically for more than a very short space of time. To deal with the many complicated emotional problems which he encounters, he uses the method of phantasy; that is to say, these problems and the people and situations they involve are represented in the child's mind symbolically, in a series of images. The phantasy of the little child is to a great extent divorced from reality, in the same way as is the adult dream; the situations and activities that occur have a 'magic' quality—they are not such as could happen in ordinary life, although they symbolize something that could or has happened. But Ruth Griffiths maintains that if the child is given suitable opportunities for expressing overtly and working out his phantasied emotional problems, in stories, drawings, etc., they will become progressively 'real'-ized. Also the child will become more fully aware of them, and they will be less dissociated from the rest of the personality, particularly from his cognitive activities.

Children, and particularly slightly older children, spontaneously express their phantasied wishes in imaginative play. Some plays of this kind have been fully examined and described by Margaret Lowenfeld in *Play in Childhood* (7). Among these children the 'magic' and symbolic elements of the phantasy were still very strong. A rather more 'real'-ized type of imaginative play is observed in the 'robber' and 'desert island' play of small boys, and the 'home-making', 'doll-nursing' and 'school-teaching' play of little girls.

The typical childish phantasy is of the kind termed by Jung 'passive'[1] phantasy (6); it represents an irruption into consciousness of some unconscious mental process, the attitude towards it of the conscious individual being almost wholly passive. Thus it is 'self-active', and in the nature of a psychic 'automatism'. It seems probable that the adolescent day-dream or 'castle-in-the-air' is of the same nature, although its 'manifest content' is much more highly socialized than that of the childish phantasy. That is to say, the underlying wishes and desires are expressed in a form which is more or less completely 'real'-ized and non-magical, although their fundamental nature is the same as that of the childish wish.

But in the older child and the adult there appears in imaginative activity the 'active phantasy' of Jung (6). Here the conscious mind actively participates in selecting elements perceived intuitively in the unconscious mental contents, developing, rearranging and synthesizing them into the imaginative construction. Conscious cognitive activity necessarily plays a large part in such processes; and more especially in the imaginative interpretation of the constructions of others. Such imaginative interpretations can frequently be obtained from older children and adults if they are shown a picture and asked: "Tell me about it." But their responses will not as a rule be based upon simple perception and description of what is portrayed in the picture. Perception will be followed by imaginative reconstruction of situations and emotional states which the individual may never have experienced except in phantasy.

It is quite clear, however, from the work of Binet (2), Ségers (8) and others, that young children are incapable of this type of 'interpretation' (the term is Binet's); the actual age at which it appears is a matter of some disagreement, and will be discussed more fully below. But at some time or other in the development of the normal individual, unconscious mental contents cease to appear in waking life in the form of 'self-active' phantasy, though still manifesting themselves in their original symbolic and 'magic' form in dreams. (They may also appear in the adolescent 'castle-in-the-air', though possibly this is to some extent a pathological form.) Their normal mode of expression in waking life is in imaginative activity (in its widest sense),[1] where they are called forth intuitively, consciously selected and finally integrated with the relevant cognitive contents. The aim of this paper is to endeavour to demonstrate this process and to trace the steps by which it occurs. To this end, a study was made of the cognition and phantasy of children ranging in age from seven to twelve or thirteen—the age range during which a transition from the 'self-active' to the imaginatively used phantasy may be expected to occur. Moreover, a number of children attending psychological clinics were also studied. It is clear from the work of Lowenfeld (7) that the phantasies of these children frequently do not develop in a normal way; and it seemed that this abnormal development, and its consequences, might throw some light on the normal processes.

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[1] This term does not seem to be a good one, since the 'passive' phantasy is characteristically 'self-active'. Therefore as far as possible we shall use the term 'self-active' phantasy, calling Jung's 'active phantasy' (*q.v.*), 'imaginative'.

The cognitive and phantasy processes to be studied centred in the main about the perception of rather complex pictures, and the verbal response given when the child was asked: "Tell me about this picture." This type of activity was chosen because it provided an objective control of the cognitive (perceptual) processes, but at the same time gave scope for imaginative activity. Also, as we shall see below, it often allowed the appearance of the childish 'self-active' phantasy. Certain other types of activity were also studied, to supplement the above. (1) and (3) were introduced to give further opportunities for the expression of phantasy; (2) was intended as a check on the child's cognitive abilities in non-verbal activity:

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(1) Verbal description from immediate memory of visually perceived meaningless, non-representational presentations;

(2) Reproduction of pictures from immediate memory, by arranging figures in the positions occupied by them in the original presentation;

(3) Free construction of pictures, i.e. the child arranged figures to make an imaginary picture.

II. METHOD OF EXPERIMENT

There were two main experiments, carried out with different groups of ordinary school children and of children attending psychological clinics for some form of emotional maladjustment or behaviour difficulty.

Experiment A. In the first experiment, a series of coloured designs, each 5 × 4 in., was presented to each child by means of a tachistoscope. This instrument is fully described in the Appendix to Part II of this paper. Here it suffices to say that it was of the Dodge type (4); and that the designs were seen binocularly, for a duration of 1·8 sec., at a distance of about 2 ft. Rigid fixation and an exact position of the head were unnecessary, but the child was given a warning signal before each presentation. He was instructed to "tell me about these pictures", and his responses were recorded verbatim.

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[1] It would seem possible that the 'school-teaching', 'home-making' and 'doll-nursing' type of play mentioned above may show one form of this imaginative activity.

The series consisted of six groups, lettered A–F, each of four designs numbered 1–4. In each group, 1 was a meaningful representation of a scene of real objects; 2 was a semi-diagrammatic representation of the same scene in approximately the same colours; 3 was much more distorted and meaningless, and usually differently coloured, but still bore some resemblance to 1; 4 consisted of irregularly shaped patches of colour. Fig. 1 is an uncoloured reproduction of B 1–4. The other meaningful pictures were as follows:

A: two boys running along a path down a hill;
C: a house with a wall and a hedge round it and a tree on either side of it;
D: a harbour with a jetty and lighthouse, and four sailing boats on the sea;
E: a man punting a boat across a river;
F: two archers in green mediaeval costume shooting.

To half the children (approximately) the designs were presented in the order: A1, 2, 3, 4, B4, 3, 2, 1, C1, 2, 3, 4, D4, 3, 2, 1, E1, 2, 3, 4, F4, 3, 2, 1; and to the other half in the reverse order. Thus the responses to half of the semi-meaningful and meaningless designs were to some extent suggested by the preceding meaningful designs; while with the other half this suggestion was lacking.

The subjects were: 20 children aged 7–8 (group I) and 20 aged 10–11 (group II) from a secondary school; and 12 aged 7–9 (group I) and 15 aged 10–13 (group II) of children attending a psychological clinic. The former carried out the experiments at their school, the latter at the clinic.

Experiment B. In the main part of the experiment, there was the following procedure:

(1) A large coloured picture, 25 by 20 in., of a scene in a farmyard (called the *Farmyard*) was presented to the child, who was instructed: "I want you to look at this picture and tell me all you can about it." All responses to this and subsequent presentations were recorded verbatim.

(2) The child was instructed: "I am going to show you a picture for a short time, and I want you to look at it carefully and tell me *afterwards* all you can remember about it." He was then shown a picture of similar size of a quarrel in a public house (called the *Tavern Brawl*), for a period of ½ min.

(3) The child was instructed: "I am going to show you another picture for a short time, and afterwards I am going to give you some little figures just like the ones in the picture; and I want you to arrange them on a piece of paper in the same places as they were in, in the picture." He was shown a picture of a milkman's horse running away (called the *Runaway Horse*) for a period of ½ min. Then he was given painted cardboard figures, replicas of those in the picture, and a sheet of paper painted in the same way as the background of the picture. After he had arranged the figures, he was instructed: "Now tell me about this picture."

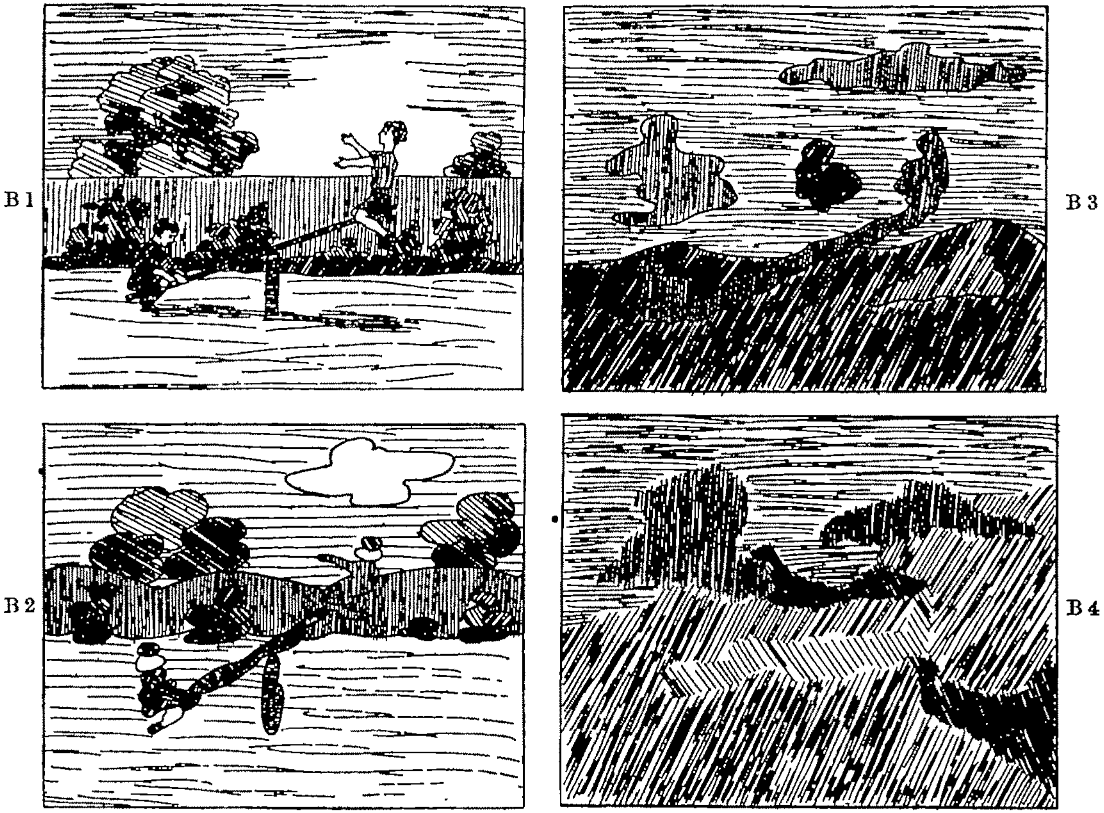

Fig. 1.

(4) The child was instructed: "Now I am going to give you some more little figures and another piece of paper, and I want you to arrange them so as to make a picture—any kind of picture you like. You needn't use all the figures—just take the ones you want." He was then given a sheet of paper painted to resemble a green field with a blue sky above, and painted cardboard figures of: Five white men (two of them with rifles), three Indians, two women, three children, four horses, six dogs, six farmyard animals, seven wild animals and six trees. (Some impression of these animals and the way in which they were arranged can be gained from Fig. 2, p. 291.) After the child had arranged them to his satisfaction (in what is referred to subsequently as a *free construction*), he was instructed: "Now tell me about it; what is it a picture of?"

The subjects were: 28 children aged 11 from a county senior school, and 14 children aged 9–11 (also one of 7 and another of 13) from a psychological clinic.

In a preliminary experiment, a picture of a medieval stag-hunt was used instead of the Runaway Horse for memory reconstruction (3); and the Runaway Horse was used for description from memory (2). The subjects were five 7-year-olds, ten 8-year-olds and six 9-year-olds from a county junior and infants' school.

These experiments were conducted in the children's schools or clinic respectively.

III. NORMAL COGNITION AND PHANTASY

(1) *Perception and immediate recall of meaningful pictures*

It was found by Binet (2) that three successive stages could be distinguished in the verbal responses of the child to the perception of fairly complicated pictures. At three years old, the child merely *enumerated* the objects which he observed in the picture. At seven years the normal child *described* the picture and its more obvious qualities; and as he grew older he included a simple description of the actions of the people depicted. But it was not until fifteen years of age that the child could really understand what was happening and *interpret* the picture fully. Binet's pictures, however, were of rather unusual and distressing scenes, the full emotional content of which would be difficult for a pre-adolescent to understand and interpret. In the latest Terman-Merrill revision of the Binet-Simon tests (9), pictures are used which are more within the child's experience, and enumeration, description and interpretation are expected from the normal child of three-and-a-half, six and twelve years respectively. Ségers (8), using a picture of a sick child, its mother and the doctor, found that interpretation was fairly well established at eleven years. Table I shows Séger's figures for the number of children at various ages giving simple enumeration, complex enumeration (characterized by more complete responses occasionally related to each other), description and interpretation. Between six and ten years, the descriptions became generally fuller; figures of people were more clearly distinguished, and the background and irrelevant details, often described by the younger children,

Table I

Age in years	No. of children giving: Simple enumerations	Complex enumerations	Description	Interpretation
3– 4	10	0	0	0
4– 5	9	1	0	0
5– 6	4	6	0	0
6– 7	2	2	6	0
7– 8	3	2	5	0
8– 9	2	1	7	0
9–10	1	0	7	2
10–11	1	2	6	1
11–12	0	2	1	7

were omitted. Though the number of children studied was small, it is clear from the Table that Ségers's figures agree well with the Terman-Merrill revision. There will be little interpretation before ten years, and an appreciable number of children will continue to enumerate until eight or nine, and even older.

It might therefore be expected that *enumeration* would occur among the younger children described in this paper, though it would not be common; and that interpretation would begin to appear among the older ones. The first expectation is fulfilled by the school children[1] of Exp. A. In three cases there was a fair amount of enumeration, but it was fairly complex: "A house with a wall and a gate and a path"; and description also occurred: "Two little boys running down a hill." Among the school children in Exp. B enumeration was more common; 50% even of the eleven-year-olds gave enumerative responses. This result was partly due to the fact that the intelligence of the Exp. B school children was lower on the average than that of the Exp. A school children. But ten of the fourteen enumerators in Exp. B had been marked A on an intelligence test; whereas of the nine children in the group who had been marked B, only four were enumerators. Clearly, then, other factors were at work besides intelligence.

It appears from Table II that the response was to a considerable extent a function of the picture shown. Picture (1), the Farmyard, was a rather static picture, lacking in exciting incident. Picture (2), the Tavern Brawl, for the eleven-year-olds, and the Runaway Horse for the younger children, was not only more exciting and dramatic; but also it depicted a central unifying incident, rather than a collection of minor details. The children were interested in the picture, and their interest aroused a more intelligent manner of response. Moreover, they were required to respond to picture (2) from memory and picture (1) from immediate observation. Thus even the slower and lazier child was obliged to formulate his response in the manner most easily remembered; and probably a brief description clinched his recollections more clearly in his mind than did a list of minor details.

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[1] The clinic children also attended school; but it is convenient to refer to the non-clinic children as 'the school children'.

Table II

		No. of enumerative responses to			
		Picture (1)		Picture (2)	
Age in years	No. in group	No.	%	No.	%
7	5	4	80	2	40
8	10	8	80	3	30
9	6	3	50	0	0
11	28	14	50	3	11

We may conclude, then, that the majority of children of these ages (see Table II) will give to a normally interesting picture, recalled immediately, a descriptive rather than an enumerative response; though there is a tendency for the slower and lazier child to resort to enumeration when neither his interest nor his powers of observation and recall are aroused. The eight children who gave enumerative responses to picture (2) as well as to picture (1) were either of low intelligence, or showed definite signs of emotional maladjustment such as will be described in § IV in Part II. For the moment it suffices to say that enumerative responses in children of these ages may be a function of emotional maladjustment, rather than of low intelligence.

It has already been noted that as the child grows older his *descriptions* become fuller, more accurate, with more description of movement, and the irrelevant detail becomes suppressed. There is, however, one particular type of description which should be noted, because it is frequent even among normal children. This for the sake of convenience is called *designation*, though perhaps it approaches the *syncretism* described by Claparède (3). I have called it designation because it gives the impression that the child is simply giving a title to the picture. It may consist of a single word, e.g. "garden", "snakes", "hill"; or of a short phrase: "Some little boys running": "Some black-coloured hills". It gives the hearer the impression that the child has, so to speak, put the matter in a nutshell; the subject is exhausted and there is no more to say about it. Whereas in complex enumeration, which it resembles to some extent, the child will drift on from one detail to another; there seems no reason why he should stop, because he may quite well begin again at the beginning and carefully repeat himself.

In Exp. A eight of the younger school children (group I) and eight of the older (group II) (40 % of each group) gave a considerable number of designatory responses. This type of response was not merely a function of intelligence, since some of these children were of good intelligence, and others rather below normal. It can hardly be called an abnormal type of response, since it occurred with such frequency. At the same time, it appeared possible that it was connected with a definite inhibition of cognitive activity, caused in some cases by mental or physical subnormality, but in others by emotional resistances of an aggressive kind. We shall return to the subject in § IV.

It is not easy to establish the point at which description is superseded by *interpretation*. It was found almost impossible to draw a hard and fast line with the rather simple meaningful pictures of Exp. A. The activities of the people in these pictures were clear and obvious; and a description of them generally included such interpretation as was possible. Certain of the clinic children, it is true, ascribed emotions and emotional expression to some of the people in the pictures, but these seemed mostly to be pure inventions—there was nothing in the pictures themselves to suggest them. However there was one type of response, that given by Katharine H. (see p. 287), which seemed to suggest the beginning of interpretation; we shall return to that shortly.

More elaborate pictures were shown in Exp. B. It is true that the picture of the Farmyard did not perhaps give much scope for interpretation, though it was expected that an intelligent child might point out that it represented an evening scene, with the cows coming home to be milked and the horses to be stabled; a scene very familiar to the country school children. In point of fact, only one response seemed to be of this kind: "Looks like a farmyard picture; . . . the man looks as if he's going to feed the cows; one of the boys is going to take the cows in; the horses look as if they're coming home from work" (May C., age 11). Five other eleven-year-olds gave responses which showed attempts to expand and interpret part of the picture, e.g. "the man's going to milk the cows", "men taking the horses back to the stables", etc. Of the younger children, three nine-year-olds and two eight-year-olds also gave partial interpretations.

The kind of interpretation that might be expected from an adult of the Tavern Brawl would be:

> It's a public house; two men (dagoes) have started a drunken quarrel and are going to attack each other with knives, but they are forcibly restrained by three sailors; the barman and barmaid are much alarmed by what is happening.

None of the children gave as coherent an interpretation as this; the best was given by one of the clinic children, Joan M. (age 11–6, I.Q. 105):

> It's a public house; lady was serving behind the bar; two gipsies with knives causing trouble; three more men settling the trouble and going to throw them out; they're throwing glasses, and the beer's upset[1] on the floor, and the stool's upset as well; a bottle on the counter; men that are helping to settle the trouble got high boots on; lady behind the counter got a shawl on and looks worried.

Thirteen of the school children (46 %) gave partial interpretations which were less complete and coherent than this. Thus it appeared that these children, even though they had some understanding of the activities represented in this picture and could give partial interpretations of them, were not able to group their impressions in such a way as to obtain a general understanding and give an interpretation of the picture as a whole.

The expected interpretation of the Runaway Horse would be:

> The milkman has been delivering milk and leaving his horse and cart standing in the road; the horse has been frightened by a barking dog and has bolted, with the milkman running after him and trying to catch him; two children playing in the road have just managed to get clear, although the little girl has tumbled in the gutter; just at this moment a youth has rushed out of the house and is stopping the horse.

Here five of the clinic children had a fairly general understanding of the picture. The best interpretation was given by Stanley H. (age 10–1, I.Q. 96):

> A milk cart and horse's been frightened away down the street and the milkman's running after him; a lady is looking out of the window, and a lady is coming out of the door, and a man come out of the door; he's stopped the horse; little girl's hurt herself; the dog kept on barking and made the horse run; the other boy's running away in case he might get hurt by the horse.

The older school children did not describe this picture. But the younger ones seemed to understand fairly well that the milkman's horse was running away, though rather fewer mentioned the fact that the man was stopping it. It appears, then, that with a suitable type of picture interpretation of a kind can begin at ten or eleven.

But one is struck by the fact that even children such as Stanley H., who seemed to understand the picture to the extent of knowing what was happening in it, did not, as most adults would, begin with the essential points and add the less important ones later. Thus with the Tavern Brawl, spilt beer and overturned tables and chairs were more emphasized than the men fighting. Again with the Runaway Horse, the women looking out of the houses were frequently mentioned before the man stopping the horse—and indeed he was sometimes omitted altogether. It seems that the child has some general idea of what is going on, but becomes confused by the detail and cannot formulate the essential points clearly; so that his response can hardly be called 'interpretation' in Binet's sense of the word. Certainly none of these responses were as advanced and coherent as that quoted by Ségers ((8), p. 729) for an eleven-year-old child. On the other hand the children's impressions of the picture were usually clear and accurate enough for them to reconstruct the picture from memory. Only five of the eleven-year-old school children (18 %) placed the figures in such a way in their reconstructions as to show they quite obviously had not grasped the essential features of the picture. It seems probable that the child when responding verbally is confused between what he actually perceives and by the phantasied elaborations of the cognized theme, as to the further activities and the emotions of the people depicted. He has not yet reached the stage at which he can make appropriate selections from the contents of his phantasies, and combine them suitably with cognitive material to produce a coherent 'interpretation' of the picture as a whole.

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[1] The 'spilt beer' was mentioned by most of the children, and seemed to impress them more than anything else in the picture.

(2) *Perception of meaningless designs*

It has been shown that the majority of the school children in both age groups used some type of description in responding to the meaningful pictures in Exp. A. The descriptions were much fuller in some cases than in others, containing sometimes more description of activities, in others more details about colour, objects in the background, etc. But these descriptions were accurate in that no actual inventions were introduced by these children. We shall see in Part II of this paper that it was characteristic of some of the clinic children to introduce inventions[1] in responding to meaningful pictures.

But when the designs were more or less meaningless and did not represent real objects and people, a great deal of invention occurred, in fact clearly it was the normal type of response to this kind of material. Particularly was this the case with the designs numbered 2 which were only semi-diagrammatic, and did resemble pictures of real objects. It will be seen from Table III that almost all the children named some object or person suggested to them by presentations of this kind. And even with the much more diagrammatic designs of the kind numbered 3 and 4, less than 20% of the responses were purely objective descriptions of shapes and colours, with no allusion to anything representational. The older children in group II were slightly, but only very slightly, more accurate in this respect. However, it did appear that the "effort after meaning" (Bartlett (1)) decreased somewhat with the less pictorial designs.

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[1] The word 'invention' is used for any feature mentioned in the response which was not derived entirely from the immediate objective presentation. Thus in certain cases inventions were wholly or in part derived from recollections of previous percepts, while in others they were completely spontaneous and original (see below).

Table III

Group	% of responses containing inventions to designs numbered 2	3	4
I	98	84	81
II	98	82	78

As with the meaningful pictures, we find the younger children responding enumeratively with strings of names: "trees, grass, rocks, clouds", etc. The designs numbered 2 and 3 were often called "men", "boys", "people", "figures", "animals". As the children grew older the responses became more descriptive and simple actions were assigned to the people and animals. These designs did of course resemble the meaningful pictures in the series, so that to some extent people and actions were suggested beforehand. But it did seem that much spontaneous invention was occurring which was not merely, or mainly, suggested by the meaningful pictures, but proceeded from subjective phantasies. It was possible to differentiate roughly between those inventions which were 'derived' directly from the meaningful pictures which immediately preceded them; those which were 'similar' to the objects in the meaningful picture of the same group, although the child did not see the latter until afterwards; and those which were entirely 'original'. The numbers are given in Table IV. It is clear that with the presentations numbered 2, 'derived' and 'similar' responses were much more frequent than 'original'; with the more diagrammatic presentations, 3 and 4, 'original' interpretations became more frequent than the other types of invention. But we cannot be certain that even the 'derived' inventions were entirely free from 'original' invention and based purely upon the child's recollections of previous percepts; indeed it is much more probable that spontaneous elaboration did enter into them to some extent. Particularly the tendency to invent men and animals seemed to operate almost throughout. Table V shows the number of responses containing invented men, animals and animated objects such as "ghosts", "scarecrows", moving

Table IV

Design no.	No. of inventions per child in Group I				Group II			
	Original	Similar	Derived	Total	Original	Similar	Derived	Total
2	1·9	6·6	6·0	14·5	1·4	7·6	6·1	15·1
3	4·4	2·1	3·1	9·6	4·4	2·0	3·1	9·5
4	6·5	0·1	0·5	7·1	7·2	0·2	0·6	8·0
Total for school children	12·8	8·8	9·6	31·2	13·0	9·8	9·8	32·6
Total for clinic children	17·3	9·3	13·4	40·0	12·0	10·3	11·4	33·7

Table V

Design no.	% of responses containing inventions of men, animals, etc., to design lettered A	B	C	D	E	F
			Group I			
2	95	70	0	0	100	100
3	70	0	0	0	25	65
4	45	0	55	0	15	50
			Group II			
2	100	90	0	0	100	100
3	90	15	0	0	45	85
4	35	0	30	5	35	55

"things" and "figures". The only group to produce no inventions of this kind was the D group. D1 was a picture of boats in a harbour, D2 and 3 showed geometrical shapes and patches of various kinds, and D4 contained irregular splashes of colour which apparently did not suggest any animal shapes—though the response, "a lot of washing blown up high", shows human influence. (The response to D3, "a flood—just the tops of houses", demonstrates clearly the compromise between the percept of D3, the recollection of D1, and phantasy elaboration.) Again, although C2 and 3 were geometric in design and produced no animal inventions, C4 contained twirly black lines which suggested "snakes", "monsters", "dragons" and "ghosts". These and other similar inventions apparently proceeded not so much from recollections of things previously perceived and experienced, but from phantasies of rather uncanny animals, moving in rather a frightening manner; such phantasies seemed ever present with many of these children. Thus "snakes", "dragons" and "ghosts" were "having a fight"; there were "two blackies—one's jumping over the other"; "a quarrel, with black men in"; "a snake on the hill and a swan flying, and the snake was jumping up to try and kill it". Even when the child could not perceive anything in the objective presentation which could be definitely named, he gave reports such as: "black things, jumping by some rocks and banks"; "black things jumping over each other ; "somebody had got a cover over them"; "something sitting on a mushroom"; "black horse thing—and two people riding it"; "they're looking at something".

Now it is clear from these responses that the children did not introduce as inventions merely the ordinary men and animals and activities familiar in their everyday life. There was obviously a suggestion of 'magic'—of creatures and activities, more familiar in dreams than in waking life. The child's 'self-active' phantasies seemed to be waiting ready to be introduced in any favourable situation with an appropriate setting. The amount of phantasy production was kept within limits by what he actually perceived. Thus both the number and nature of these phantastic inventions were limited. Very few of them seemed to have lost all trace of resemblance to the original presentation, though Michael W.'s response to B 2 (see Fig. 1) came near it: "An aeroplane and three little men; green and all colours, and a path; they've got a piece of rope and it was tied on the tail of an aeroplane—I think it was an aeroplane, or the shadow of an aeroplane." We shall find more responses of this kind among the clinic children. But since the phantasy material was in general limited, it is not possible at this stage to analyse its nature and content in any detail. It can only be said that:

(*a*) It deals in the main with living creatures. These are sometimes people or familiar animals; but often more or less 'magical' animals (snakes, ghosts, dragons, etc.); and sometimes mysterious and nameless "things" or "figures".

(*b*) These creatures are often engaged in some rather dangerous or suspicious occupation—"fighting", "cutting each other", "jumping up at" or "over each other", "running after each other", or just "looking at each other".

But most of the children realized to some extent that they were introducing inventions into their responses, and that the latter were not exact accounts of the objective presentations. It is true that one or two of the younger and least intelligent children produced straightforward inventions without any verbal qualification to indicate that they were aware of anything incongruous or not strictly objective in their response. Two others also gave straightforward inventions, but these seemed to proceed from superficiality of observation, resulting from lack of effort or of co-operation; that is to say, the perceptual element was reduced to a minimum and the response was based almost entirely upon phantasy. This phenomenon appeared more clearly among the clinic children.

The slightly older children qualified their inventions by using the terms: "Some very funny things"; "queer"; "crinkly"; "squiggly"; etc. This seemed to indicate their recognition that the objective presentation did not contain exact representations of the men and animals which they reported. At a rather older stage "like" and "sort of" were introduced as qualifications, and later the response might take the form of a query: "Was it a pathway up by the hill?" Or sometimes the response indicated that perception and phantasy existed side by side, but separated, with no interrelationship between them; thus: "Another blue sky and some black streaky stuff and lots of other colours all round"; "yellow and red and brown and black and all colours, crinkly things, like dragons again".

There were those who introduced little or no invention of any kind. Two such cases were superficial observers who responded perfunctorily with "design", "pattern", "blocks", "lines", etc. Some of the older and more intelligent children, particularly if they were of a rather suspicious and critical turn of mind, called the designs "bad painting", "just a muddle of colours", "painted picture—hadn't been finished", "child's drawing". This seemed to indicate that there was a tendency to introduce phantasy which was repressed as being incongruous with actual perception; but this tendency was still sufficiently strong to inhibit clear perception of the objective presentation. There were, however, two children who seemed to have repressed almost all phantasy, and gave long and accurate descriptions of the colours and shapes in the presentations. Such children were of the careful, methodical and 'scientific' type; but at the same time they showed tendencies to over-conscientiousness and a rather compulsive use of verbalization. We shall return to this type later.

Finally there was one child who gave evidence of a really satisfactory type of reconciliation between perception and phantasy. Katharine H. (age 12–2, I.Q. over 136) was one of the clinic children, not one of the ordinary school children. She alone realized that the designs formed a progressive series from meaningful pictures through semi-meaningless designs to patches of colour. She understood the way in which successive presentations became more or less meaningful, thus: (A 1–3) "Two boys running down a hill"; "The boys were changed and all squiggled"; "Looks as if they were beginning to turn into trees". One might say that she had reached the stage of real interpretation. For here the objective configuration and the imaginative invention were reconciled and synthesized, on a basis of rational understanding.

(3) *Free construction*

So far we have dealt with cognitive activities which were more or less closely constrained by the necessity of reproducing some particular objective presentation. But in Exp. B the school and clinic children were given an opportunity of exercising both their purely cognitive abilities and also their phantasies in a less limited fashion. It is true that limitations were imposed by the nature of the material given them for construction. But the actual selection and arrangement of this material were determined by their tendencies to invention and by the boundaries imposed by ideas of reality derived from knowledge and understanding of the world outside.

There appeared to be three main stages in the development of these constructions, in general succeeding each other as the child's age increased:

A. The youngest school children seemed to make no real attempt at construction, but to endeavour to crowd as many of the figures as possible into the given space. This was not done simply at random, but quite carefully, apparently with some idea of pattern in the arrangement (i.e. the figures were sometimes placed in long straight rows across the background). But it was not possible to see any ulterior significance in this pattern. Thus A seemed to correspond to the stage of enumeration in perception. Four of the seven-year-olds, two of the eight-year-olds and one nine-year-old (one-third of the younger school children) produced such constructions.

B. At the next stage, selection of some kind took place. Either a large class of figures was rejected, e.g. all the wild animals, or certain members of each class were selected, and these were grouped in a way which was meaningful to the child, and which he could sometimes describe verbally. Sometimes the significance of the grouping was fairly obvious to the experimenter, at other times more difficult to grasp. But the arrangement did not form a coherent whole; it was rather a collection of segregated figures or groups, each group varying in number from two figures upwards. This stage seems to correspond to description and partial interpretation in perception. Eleven (42 %) of the younger school children were at this stage, and nineteen (68 %) of the eleven-year-olds.

C. At the highest stage the figures were arranged in a single coherent whole—what in fact we should call 'a picture'; and this was given a name and a rational description. Three (14 %) of the younger school children and nine (32 %) of the eleven-year-olds had reached this stage.

It seemed that the transition from stage A to stage B was mainly a function of mental age. At stage A the child's phantasies were too incoherent or insufficiently related to his cognitive functions to find expression in this form of free construction. Either the child was not fully aware of his phantasies, or his phantasies were dissociated from his cognitive activities and the latter insufficiently developed for utilization in dealing with the material provided.

At stage B the child was able to make use of his knowledge of the real world in cognizing the material, and his cognitive ability in manipulating it; also he could use the latter to express his phantasies. But he could not yet make use of phantasy material actively, in unified imaginative construction. Thus we find that some children inclined more to the use of isolated pieces of recollected experience of the familiar external world. Others used the experimental material more or less in symbolic form, to express their phantasies; while yet others seemed to hover uneasily between the two modes of expression. From the first type came productions which appeared similar, and were described in similar fashion, to the partial interpretations which were made by the child in his observation of and response to pictures. These consisted of a number of groups of people, animals, etc., each of which had a meaningful relation. Moreover, there was a vague coherence or 'fittingness' between the groups—that is to say, they were not wildly inconsistent with each other; but there was no complete and logical relationship between them. They were described in a set of consecutive but unrelated verbal statements; thus Arthur H. among the eleven-year-olds gave:

> In the field all the cattle feeding and racing about. A farmer walking with a little girl and a dog. A man shooting with another dog. Two horses racing along. Two pigs fighting each other. Two sheep and a man walking.

Here it appears that the child had a general idea of a country-side with people and farm animals engaged in normal activities; but these were only very loosely interrelated. No imaginative process had united the separate items into an interpretative whole.

But there were frequent cases in which there was considerable inconsistency between the separate groups. Thus Douglas P. gave:

> Driving some horses down to a field. A dog chasing a boy. A boy with his elephant. A man and a woman talking. A boy calling his dogs.

This type of description was sometimes greatly elaborated with much more detail and a larger number of segregated groups. Sometimes it was more obviously incoherent, as in the case of Rosemary P.:

> A man running after a horse trying to catch it; some people watching on. A man trying to shoot something[1] near the trees. A boy running away from it. A lady going to market.

Again in some cases the child found it easier to give a general name such as 'Zoo' or 'Jungle', to a number of unrelated groups. Thus John P. produced seven or eight groups, of men shooting animals, dogs chasing sheep and cows, a man chasing a horse, a child with a woman and a dog, etc.; and this production was called, after much hesitation, "Game hunting". It seemed probable that some of these items were suggested from previous experiences, and were based upon the perception and cognizing of something existing in the external world. Other items had arisen in phantasy, possibly from association to the more normal items. But logical coherence and interrelationship were lacking because the child was unable to make active use of the phantasy elements.

There were other cases in which there seemed to be a definite line, or lines, of demarcation between different parts of the construction. Thus Kenneth B. described his construction as:

> Wild animals at the back, and a man going after horses, and some sheep and cows and people going across the field.

The wild animals, and four men attacking them, were arranged as a horizontal strip across the back of the field; in front of this in the centre was the man going after the horses; on one side, a man, woman and child and two dogs; on the other a man, a dog and some sheep. Here it appeared that the phantasies about dangerous wild animals and the attack on them were pushed to the back of the mind, while ordinary activities (man and horses) and peaceful occupations (people, dogs, sheep) were segregated from them in the foreground. In other cases the isolation of the phantasied danger was evident from the fact that the child described the rest of the construction, but made no mention of what was clearly an important part of it. Thus Leonard B. called his production, "Meadows with sheep and horses and a man about", but did not allude to a man shooting a bear, in the middle of the back of the field. Again Bessie M. described hers as, "People have come to see that girl; and that's the keeper with his dog", making no mention of the line of wild animals placed across the field behind the people and the dog.

[1] A rather large and ferocious looking cat, several times taken for a wild animal.

Finally we may include among the stage B productions one by Monica B. in which the dangerous phantasies seemed to be gaining the upper hand. It was called simply, "Jungle, with some animals in it"; but the wild animals were arranged in a semi-circle round the lonely figure of a little girl of about the same age as Monica!

This construction might almost be grouped among the unified constructions of stage C, but that it showed a clear lack of contact with reality. All those assigned to stage C were to some extent 'real'-ized, although they may be divided into those which were mainly derived from objective experience, and those which contained a large imaginative component. Of the former, the most intelligent and best arranged was made by Kathleen B., who described it as, "Field with some lambs and some horses, and a man going to bring in the horses". But in the main these productions were rather pedestrian and unimaginative, and possibly derived to some extent from the picture of the Farmyard, which the children had seen previously. Indeed it is possible that some of them contained so little of the unifying imaginative factor that they should have been excluded from this stage. The rather popular 'Jungle' groups gave more scope for imagination. These in general took the form of a group of men shooting or chasing wild animals. But sometimes the men were right in one corner of the picture (Tony P. (reproduced roughly in Fig. 2), John C.); and sometimes they were themselves being chased from behind by other animals (Stanley N.). Here clearly the dangerous and fear-provoking forces were in the ascendent. Sometimes the forces were about equally balanced, however (Derek W., Vincent B.). It may be

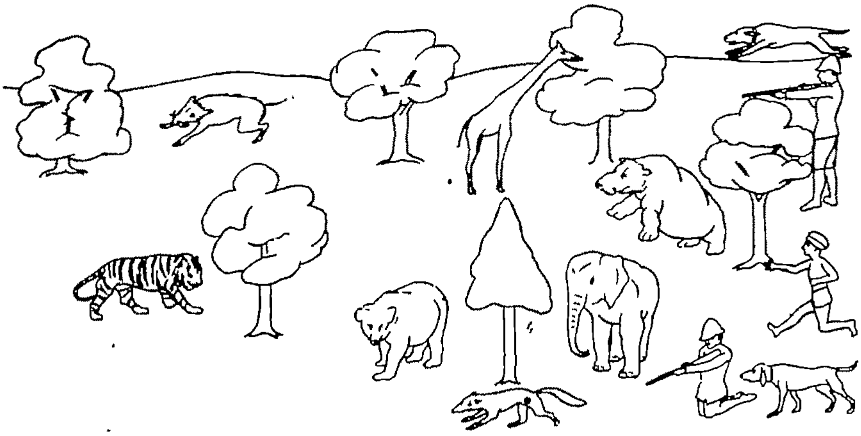

Fig. 2.

noted that Stanley N. was the only one of these children to mention that the men were themselves being attacked by animals. There were also two cases in which the child's own aggressiveness seemed to be finding expression. Thus Donald M. made a "field with horses, a man waving a whip and frightening them, with a dog"; and Rosemary J. produced: "A fox, and some men chasing it" (there were five men, a child and three dogs against one fox).

It is possible to compare stage C in free construction with the interpretative stage of perception, though at first there may appear to be little justification for this comparison, since in interpretation the child is compelled to consider carefully the nature of the objective presentation in order to interpret it correctly; while in free construction he can use his own imagination freely. But we have already pointed out that even in free construction the child may definitely be limited by his ideas of reality, i.e. of the kind of activities and situations which are characteristic of people and wild and tame animals in the real world. Moreover the child at stage C is not only observing these limitations, but has also synthesized his construction into a coherent whole. In the same way when he gives an interpretative response to a perceived picture he has 'imagined' into it ideas, situations, activities, which are not actually represented there; he has selected the appropriate ones in the light of his experience of the real world, and has synthesized them into a coherent whole. Thus we conclude that the characteristics of these stages which demonstrate the functioning of the fully developed powers of imaginative interpretation are:

(1) The accurate perception and cognition of the objective situation, i.e. what is normally called perception (see Vernon (10), pp. 5, 6);

(2) The phantasying of more or less emotionally charged situations suggested by, and associated from, the percept;

(3) The active selection from this phantasy material of what is relevant to the percept *and* what is consistent with knowledge and experience of the real world (i.e. in accordance with the 'reality principle');

(4) The final synthesis of cognized and phantasy material so that the response consists of an integrated whole containing what is essential and relevant both of the immediate percept and of the appropriate phantasy material; less important features being relegated to the background and unimportant detail being altogether omitted.

In perception of a picture the response is determined to a considerable extent by (1). In free construction, (1) is limited to the perception of the figures and the background sheet of paper, and the response is determined to a much greater extent by (2). But in both cases, (3) and (4) are essential in a response at the stage C or interpretative level.

Stage B and the level of description and partial interpretation are generally characterized by (1) and (2) above, but (3) and (4) are more or less incomplete. That is to say, as a rule perception is fairly accurate, and can be reported clearly, as in description. 'Self-active' phantasy also occurs when there are inventions, and in partial interpretation; this can be described verbally, but cannot be unified with the perceptual material. The response may thus contain perceived and phantasied material side by side, either unrelated or in conflict with each other. Since the phantasy is 'self-active', it is not selected or controlled cognitively; thus no real synthesis between cognitive and phantasy elements can occur.

But it appeared that the absence of imaginative interpretation was to some extent a function of the objective presentation. Clearly it would be difficult for even an intelligent adult to produce an imaginative interpretation of a picture which was in the main a collection of minor details, and gave little opportunity for the exercise of imagination. And with such a picture, the child may confine himself to objective description. Or such description may occur when the child is for some reason or other unable to express his phantasies, or has completely repressed them. On the other hand there are cases, and we shall describe more of them in Section IV of Part II, where accurate perception and contact with reality are at a minimum, and the response is mainly the result of 'self-active' phantasies, containing little of the perceptual element. Thus the appearance of descriptive responses or stage B constructions may be due to:

(*a*) Lack of maturation through age; i.e. they are characteristic of the normal child of less than about ten years;

(*b*) Unsuitable perceptual material;

(*c*) Failure of cognitive ability or phantasy, or both, to develop normally through some emotional maladjustment.

It is less easy to understand the nature of the response at the stage A and enumerative levels. Verbal communication is difficult at this level. Thus we cannot be sure if the phantasies are themselves so incoherent that the child is not fully aware of them; or if the cognitive abilities are insufficiently developed to be used in expressing them. Clearly the careful but apparently meaningless arrangement of objects in free construction is closely analogous to the listing of names of objects in enumeration. We can only conclude that the child is trying out its recently developed cognitive skills (since naming objects is the earliest form of language) without at present being able to use them for the expression of symbolically imaged phantasies. As soon as the child begins to draw inferences from its observations as to the actions and emotions of the people, etc., depicted in a static picture, and to formulate these in words, a real link has been formed between conscious cognitive activities and phantasy processes.

It may be objected that the five-year-old children studied by Griffiths (5) were able to express their phantasies in words and in other activities such as drawing. Thus it seems that the child *can* use such techniques for expressing phantasy even when quite little. But apparently only an older child can use them to describe synthesized phantasy and cognitive material. The phantasy material can appear in words and in constructive activities, but it is much more difficult for the child to use it deliberately, to employ it consciously and only when it is fully congruent with his perceptions and knowledge of reality. We may suppose, and this would agree with the conclusions of Griffiths, that it is by expressing his 'self-active' phantasies in words (such as we have encountered in the 'partial' interpretations) that the child becomes more fully conscious of them; and is thus enabled to observe them, and finally to select and make use of them in imaginative interpretation and construction.

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SOME EXAMPLES OF CONSTRUCTIVE THINKING AMONGST GREENLANDERS

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From the Cambridge Psychological Laboratory

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I. PLACE AND TYPE OF SUBJECT

THE experiments about to be described were carried out among the native population of the Umanak district of West Greenland, during the summer of 1938. In this district, the admixture of European (especially Danish) blood is so strong that it is doubtful whether any pure Eskimos remain. The natives call themselves "Greenlanders", a term which will be used throughout this paper.

Under the Danish administration, whose ideal is a well-planned and carefully sheltered transition from the original type of Eskimo community to a form of European civilization suited to the country and its inhabitants, all West Greenlanders are Christianized, have churches, schools and a medical service, and enjoy some degree of representative self-government. Most of the subjects who volunteered for these experiments had attended, or were attending, primary schools where reading and writing in the Greenlandic dialect of Eskimo, a little Danish, arithmetic, history and geography are taught, and some, such as administrative employees and 'catechists', or native teachers, had received a secondary education.

The experiments were carried out in Umanak, the administrative centre and chief trading station of the district, and in the two outposts of Igdlorssuit and Qaersut. The two hundred Greenlanders in Umanak are much in contact with white people, for from fifteen to twenty Danes reside there permanently, and in the summer ships from Denmark call at frequent intervals. In each of the outposts, on the other hand, there is only one Dane, the trader, and the majority of the population seldom have dealings with, or even see, any white man other than him.

II. AIM OF THE EXPERIMENTS

The experiments have two aims, of which the first is the more important:

(i) It is contended by Lévy-Bruhl (1) that primitive people think differently from normal civilized persons, in the sense that their mental processes, as such, are essentially different. The way in which primitive man interprets the universe of his experience is contrasted sharply with the manner in which modern scientists and philosophers deal with this problem, and the conclusion is reached that primitive man's thinking is pre-logical, emotional, mystical and self-contradictory. The very most which Lévy-Bruhl succeeds in proving, however, is that primitive man does not employ certain *logical criteria* of truth and falsity, and no real evidence is brought forward to show that his thinking, as a mental process, is *psychologically* different from that of the scientist or philosopher. The present experiments are designed to investigate whether or not, when normal civilized persons and a relatively primitive people are faced with a certain type of problem, they think in exactly the same way. Lévy-Bruhl, of course, carefully confines his statements to primitives untouched by European culture. Of the Greenlanders, however, Schultz-Lorenzen has written ((2), p. 210): "The Eskimo mind has kept its peculiar primitive character unchanged...it holds its ideas and groups of ideas as juxtaposed factors. It is unable to join them in a unity.... The Greenlander has no difficulty in keeping in his mind considerable contradiction and contrasts."

(ii) Experiments of a similar type (3,4), carried out with various social groups in England, have shown some interesting differences in the nature, frequency and distribution of conventionalized opinion concerning social behaviour. In some of the Greenland experiments, therefore, the opportunity was taken to invent and use material likely to elicit from the Greenlanders some expression of opinion concerning their attitude to, and working relationships with, Europeans.

III. MATERIAL AND METHOD

The material used consists of a number of "incomplete situations" (see (3)), in each of which the issue turns on the actions of two or more persons or groups of persons. The situations, examples of which are printed below, are left incomplete, but a number of clues are given which indicate possible completions. Subjects are asked to state what they think is most likely to happen in each situation, and their answers are subsequently analysed and classified.

The advantages of this method are: (1) it is *objective*, and does not demand introspection on the part of the subject—an essential requirement when dealing with primitive people; (2) it is *realistic*, presenting situations which demand *practical* rather than abstract scientific thinking, which readily interest all types of subject, and about which most subjects feel justified in expressing an opinion: (3) it eliminates the danger of becoming involved in irrelevant *logical* questions, for it uses situations of which no one of a number of possible solutions is 'more right' than any other. Thus any two solutions may be widely different, or even diametrically opposed to one another, and yet equally relevant to the psychological study of the mental processes from which they result.

IV. EXAMPLE I

(i) *Material*

The Three Hunters

It was winter, and three men were sitting talking in a *fangsthus*[1] far from their settlement. One said: "Joab is a very bad man." "Yes", said another, "since he

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[1] Hunting-house.

> came to our settlement from South Greenland six months ago he has done all manner of evil things. He has told many lies, he has stolen, he has been cruel to our children, and a nuisance to our women. And only last week he tried to kill my best friend. It is time for us to get rid of him." "That will not be easy", said the third. "Already we have asked the Governor to take him away, but nothing has been done."
>
> For a while they sat silent, and then the man who had first spoken said: "We could kill him." "It is wrong to kill", said the second: "we should be punished." "It would be good for all of us if he were dead", replied the first man: "I myself shall kill him, if ever I can do so without being found out." "We could kill him here, and no one know of it, for this place is very lonely", said the third man. "Joab never comes here", said the second man: but even as he spoke the door opened, and Joab himself entered. "I have lost my sledge and my dogs", he said. The three hunters looked at him, and saw that he was already nearly dead of cold.

The question in this instance is *What will the three hunters do?*

(ii) *Method of presentation*

This story was presented to 100 subjects—43 in Umanak, 36 in Igdlorssuit, and 21 in Qaersut. Of the total number, 53 were men, 21 women and 26 children under the age of 14. In Igdlorssuit each subject was taken individually, the interpreter both reading the story and recording his subject's answer. In Umanak and Qaersut all results were obtained from group experiments, in which the interpreter read the story (usually about two or three times) and each subject wrote down his (or her) own answer. All subjects were allowed to refer to the text of the story if they wished to do so.

(iii) *Analysis of results*

Table I shows the distribution of opinion in each settlement, and for the total number of subjects, in answer to the question, "What will the three hunters do?"

Table I

	Igdlorssuit		Qaersut		Umanak		Total
	No.	%	No.	%	No.	%	no.
Kill	7	19	3	14	12	28	22
Not kill	27	75	17	81	29	67	73
Undecided	2	6	1	5	2	5	5
Total	36		21		43		100

The distribution in each settlement group is remarkably similar. Of the total number of women and children, however, 33 % predict that Joab will be killed; of the total number of men, only 10 %—which probably reflects a real difference of opinion.

Of those who predicted that Joab would not be killed, many stated positively what the three hunters would do. Their predictions are classified in Table II without differentiating for sex, age or settlement grouping.

Table II

Help unreservedly	15
Supply with bare necessities and then let him depart	5
Be reconciled	4
Admonish, warn, threaten	4
Punish, beat	2
Consult magistrate	1

One subject distinguished between what *would* happen and what *ought* to happen. "The three hunters", he said, "will agree well enough with Joab, though it is not right that they should do so." Only one subject predicted *individual* rather than *group* action. He thought that the three hunters would feed and clothe Joab, and send him on his way. Then the first man would pursue and slay him, in accordance with his avowed intention.

In addition to stating what would happen, many subjects stated the motives which they thought would determine the line of action taken by the three hunters. These motives are classified in Table III.

Table III

	Igdlorssuit	Qaersut	Umanak	Total
Ethical:				
It is wrong to kill The bad custom of killing has disappeared in Greenland Christian people don't kill	16	2	4	22
It is good to help Greenlanders always help people in distress To help is to act like men Hunters always help each other in times of danger	9	3	—	12
Psychological:				
Joab's plight evokes pity Anger and hatred vanish in time of danger	13	4	7	24

The greater freedom of expression enjoyed by the Igdlorssuit group is clearly evident in Table III and suggests at once that, whenever possible, this method of individual presentation and oral response should be used with relatively primitive people.

What is most remarkable about the Igdlorssuit answers, however, is a 'group self-consciousness' which in some instances amounts almost to 'group self-righteousness'. This self-consciousness appears to a lesser extent in the Qaersut answers, and is entirely absent from the Umanak group.[1] Igdlorssuit subjects, men, women and sometimes even children, do not say simply, "It is wrong to kill", "Hunters help each other"; and so on, but far oftener, "We *know* that it is wrong to kill", "We Greenlanders are Christians and Christians don't kill", "We Greenland hunters help each other", and so on. Indeed, the attribution of irregular behaviour and murderous intentions to Greenlanders brought out from some subjects a real rebuke. One Qaersut hunter says, "Men cannot kill each other—as *you* know also". The head man of Igdlorssuit says, "The beginning of the story is all right, but the rest is bad and thoroughly wrong. I have never heard anything like this in any place". Another old Igdlorssuit hunter welcomes the chance to correct wrong opinion: "If strangers believe such things of Greenlanders, then it is very bad and nonsense. But it is good that I have read this story and can answer it."

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[1] It appears in Umanak, however, in response to specific questioning (see Example III below).

(iv) *General conclusion from Example I*

The speed and confidence with which all subjects made their predictions, the fact that these predictions ran along easy, stereotyped lines, and the high percentage of agreement, all indicate that the presentation of the material immediately caused reference to be made to certain widely accepted generalizations.

It is apparent, especially from the forceful manner in which they are explicitly formulated, that the belief in these generalizations, and their utilization in the present instance at least, are accompanied, and probably to a large extent determined, by strong emotional factors. This is most clearly seen when their implied contradiction arouses actual resentment.

Once these generalizations have been accepted as relevant to the presented material, they have such 'weight' as almost completely to dominate all subsequent 'thinking', and to exclude careful consideration of the trend of the story and the significance of the data which it contains. This is perhaps most clearly seen in the last paragraph, when the generalizations evoked by the mere mention of Joab's sorry plight are such as to make practically all subjects overlook the fact that this same sorry plight makes it all the easier for the hunters to carry out their professed intentions.

V. EXAMPLE II

(i) *Material*

Tunersuk

"A month ago", said the young hunter, "Tunersuk went away in his *kayak*: none of us have ever seen him again." "Perhaps he had an accident", said the trader. "That is not likely", said the young hunter; "he was the best *kayak* man in the whole settlement." "He certainly was a fine hunter", said the trader, "but he would not fish or take sharks, and so lost much money." "Did you not often advise him to fish?" asked the catechist. "Oh, yes", said the trader, "but he always replied that fishing was work only fitting for women and boys. He was proud." "I should say that he was just lazy", replied the catechist. The young hunter looked angry: "No", he said, "he was not lazy: he thought it best to live like the old Greenlanders: and he used to say that it would be better for Greenland if the white men had never come." "Nonsense!" said the catechist: "where would we all be now without the white men?" "*You* certainly would not be here", replied the hunter.

The trader spoke again: "It is foolish to quarrel about things which cannot be changed. These last two years Tunersuk suffered every kind of misfortune—he lost his wife, his dogs, his sledge and his *kayak*. Then he himself fell ill: I think that he knew that he would not live long. Then he began to hate the white men and to blame them for everything. I offered him a boat and tackle to take sharks, but he angrily refused; and when I gave him money he threw it back at me. After that I could do nothing more."

"He came to me before he left", said the young hunter, "I was always his friend. He told me that he was going away for ever because there was nothing more for him to do here." "I wonder what has happened to him", said the trader. "I believe", said the young hunter, "that he is now a *kivitok*.[1]"

The catechist laughed, but rather uneasily.

The questions in this instance are:

(1) What do you think happened to Tunersuk?

(2) Which of the three men, trader, hunter, and catechist, do you like *most*?

Which do you like *least*?

(3) What do you think of Tunersuk's behaviour and opinions?

(ii) *Method of presentation*

This story was presented to 88 subjects—24 in Igdlorssuit, 21 in Qaersut and 43 in Umanak. Of the total number, 47 were men, 29 women and 12 children under 14 years of age. All results were obtained from group experiments conducted as described above (Example I, p. 298).

This material differs from Examples I, III, IV in demanding a *retrospective* rather than a *predictive* judgement. It will be seen, however, that this difference is not so essential as to affect the general manner in which subjects deal with the data presented. The 'supernatural element' was introduced because it is maintained that a belief in the traditional Eskimo 'demonology' persists even among the more enlightened West Greenlanders, and can affect both their thinking and their behaviour.

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[1] A man who, having fled the settlements, maintains himself in a demoniac state in the mountains.

(iii) *Analysis of results*

Table IV classifies and shows the relative emphasis on the answers to question 1, "What happened to Tunersuk?"

Table IV

	Igdlorssuit			Qaersut			Umanak			Total	
		No.	%		No.	%		No.	%	No.	%
Became *kivitok*		6	25		6	28		29	67	41	47
Did not become *kivitok*:											
Perished in *kayak*	14	16	67	12	14	67	12	14	33	44	50
Insane (but *not kivitok*)	1			1			1				
Stayed away, alive and well	1			1			1				
Doubtful		2	8		1	5		—	—	3	3
Total		24			21			43		88	

The distribution in the two hunting settlements is very similar, about 25% of each group saying that Tunersuk turned *kivitok*. It is interesting that in the more 'civilized' community of Umanak this percentage should rise to 67–55% of the men, 64% of the women and nearly 100% of the children. On an over-all count, nearly 50% give a 'superstitious' explanation of Tunersuk's disappearance; but if subjects under 14 years of age are eliminated (as they probably ought to be, for they occur only in the Umanak group), then the percentage of 'supernaturalists' is 40%, of 'rationalists' 60%.

The division of opinion was very marked, practically all the 'supernaturalists' saying that Tunersuk *certainly* became *kivitok*, and all the 'rationalists' that he *certainly* did not. Many of the more enlightened and intelligent Greenlanders are most emphatic supernaturalists, e.g. the 'catechist' (native teacher and lay preacher) of Qaersut, who says: "Tunersuk certainly became *kivitok*, after the old Greenland custom." The headman of Igdlorssuit prefers to suspend judgement: "This is the old Greenland custom", he says, "I can well believe that a human being can turn *kivitok*, though I have never seen any such thing." One of his brothers, on the other hand, states that, "no one can become *kivitok* in *our* time", in contrast to yet another brother, who, along with four Igdlorssuit and five Umanak people, believes that Tunersuk must have turned *kivitok* "because he *said* that he was going to, and *could do so if he wished*".

Questions 2 and 3 concern only the subsidiary aims of these experiments. Table V brings out, with small variations, for each settlement, a fairly definite order of preference.

Reasons for preference are seldom given. They do appear, however, among those who, like the hunter, are Greenlanders of the 'old-fashioned', independent type. One of them says, "It *is* better to hunt than to fish, and there are still enough seals for all men to be hunters"; and another (from Umanak) believes that "all hunters have a mind of their own"—perhaps a subtle criticism of his fellow Greenlanders in the employment of the Danes! So few subjects answered the second part of question 2 that their answers are not worth tabulating.

Table V

Which of the three men do you like most?	Idglorssuit (24)	Qaersut (21)	Umanak (43)	Total (88)
Trader	7	11	19	37
Catechist	9	1	13	23
Hunter	2	7	11	20
No preference expressed	6	2	—	8

Table VI classifies the types of answers received to question 3.

Table VI

What do you think of Tunersuk's behaviour and opinions?	Igdlorssuit (24)	Qaersut (21)	Umanak (43)	Total (88)
Good	8	5	8	21
Bad	7	9	31	47
First good, then bad	1	—	2	3
Not evil, but mad	1	—	—	1
Indefinite answers	1	4	—	5
No answer	6	3	2	11

No justification of Tunersuk's behaviour as good was attempted, though several subjects considered him good "in spite of everything". Many reasons were given for condemning him as bad; "he turned *kivitok*"; he was lazy, improvident, did not live lawfully, was not good-hearted, was too easily offended and so on. The only explicit reference to Tunersuk's hatred of the white man is made by the headman of Igdlorssuit and his brothers, who find it a fault in Tunersuk that "he was not friendly to the white men, who are our helpers".

(iv) *General conclusions from Example II*

This story aroused no criticism of the type levelled against Example I (p. 300, above). On the contrary, in all three settlements the Greenlanders said: "Of all the stories which you have told us, Tunersuk is the best, for it describes the kind of thing which might happen to us Greenlanders at any time." At the mention of *kivitok*, and occasionally while subjects were recording their answers, there was some laughter, but it was excited and semi-apologetic rather than scornful, and therefore there is good reason to assume that in general the emotional reaction to the story was such as to encourage sincerity.

The presentation of the material evidently suggested immediately two sharply contrasted types of explanation. Of the two, the 'supernatural', probably on account of its emotional associations, has by far the greater 'weight', for although it is *accepted* by less than half the number of subjects, it is *mentioned* and *expressly rejected* by nearly all the others.

'Thinking about' the presented situation did not entail any very careful consideration of specific data, but resolved itself into a question of accepting or rejecting a conventionalized and formerly universally accepted type of explanation. Acceptance and rejection, as shown by the speed and confidence with which subjects made their decision, and by the kind of reasons occasionally expressed (p. 302, above), seem to have been largely pre-determined by factors quite removed from the presented situation; and where actual reference is made to the text, the acceptance of the *kivitok* hypothesis causes ten subjects to twist Tunersuk's words to suit their case (p. 302).

It appears, therefore, that in spite of the one demanding a predictive and the other a retrospective judgement, the subjects' modes of dealing with both Example I and Example II display much the same general characteristics.

The value of the answers to questions 2 and 3 will be considered later in a general discussion of the secondary aim of these experiments.

VI. EXAMPLE III

(i) *Material*

The Famine

Many years ago ten Englishmen came to Greenland on an expedition. They brought much food and equipment, because they expected to work for two years. The place where they built their house was very lonely and difficult of access, and the only people there were some twenty Greenlanders who lived in a small settlement ½ km. north of the white men's house. These Greenlanders told the Englishmen that sometimes in the winter they were for long periods cut off from all communication with other settlements.

The first year all went well. The Greenlanders had good hunting, the white men collected rocks and flowers; they flew balloons; they dug up old houses and in general did all the peculiar things which white men do, and seemed highly pleased with themselves and their work. Frequently they asked the Greenlanders to help them in one way or another, and the Greenlanders were always willing to do so. The Englishmen paid good wages and gave many presents; and they always made the Greenlanders welcome in their house. And when the Greenlanders asked them to a 'dancé-mik' or a 'café-mik' they always seemed glad to come. Of course the Greenlanders, being poor, could not give such fine presents as the Englishmen, but none the less many of them gave small gifts which the Englishmen seemed to prize highly.

One day, after a Greenlander called Jakob had brought such a gift and gone away, one of the Englishmen said to his companions: "You know, I believe that Jakob, and most of the Greenlanders, give us these gifts only because they hope to get a better one in return." "That's a mean thing to say", replied the man who had received the gift, "and I'm sure that it's untrue and unfair to Jakob and all our Greenland friends." "You are very generous, but a little simple-minded", said the first ironically. "For my part I don't believe that white men and Greenlanders can ever understand each other, or be real friends. The Greenlanders do not like us *for ourselves*, for *what we are*, but for what they can get out of us; and if we'd nothing to give these people here then there would be a quick end to this so-called friendship of ours." "You are wrong", answered the second man, hotly. "After all, white men and Greenlanders are human beings, made after the same pattern, and living much the same sort of life—eating and sleeping, loving and hating, crying and laughing, growing up, growing old, and all meeting the same fate in the end. And in these things are found the beginnings of true friendship. I am sure", he added, "that Jakob and I are real friends; that he likes me as I like him, and that he would stand by me in any danger." "Perhaps; perhaps not", answered the other. "I'd like to see what would happen if it was a question of our lives or the lives of our Greenland friends. I bet you we'd be left to shift for ourselves."

The very next winter his words were put to the test. The early months were wild and stormy, and little hunting was possible either on sea or land. The new ice was broken over and over again, and everywhere in Greenland the people suffered much from cold and hunger. The Greenlanders in the little lonely settlement beside the white men suffered most of all. They had no trading-store; they could not travel to any other settlement for help, and in the end it was the white men who saved them. They shared with them the food they had brought from Europe, giving to every one that was in need, and taking care above all that no little child should ever go hungry.

At last the ice came, and the Greenlanders began to get a few seals—but not many, scarcely enough to feed themselves. The white men could not hunt, and one day they came to the Greenlanders and said: "Our food is nearly finished because we shared it with you when you had none. Can you help us now?" The Greenlanders did not know what to do. "We ourselves", they said, "have just enough from day to day, and no one knows but that to-morrow we may have nothing. Already some of us are weak and ill from lack of food." When the leader of the Englishmen heard this, he said, "Do what you think best. We shall return in three days' time to hear your decision."

They went away, and the Greenlanders began to discuss the matter among themselves. "We ought to help them because they helped us", said some. "Perhaps", said others. "But you forget that when they helped us they had plenty, and both they and we confidently expected that good hunting would come soon. Good hunting has not come. If we share our food with them now then it is highly probable that both they and we shall perish. But if we keep what we have, then we may live. It is better that some should live than that all should die; and it is natural that we should save ourselves."

"It may be *natural* to save ourselves", said the catechist at this point, "but it would be cowardly and ungrateful and dishonourable. You remember what the Bible says—'Greater love than this hath no man, that he lay down his life for his friend'." "Yes. For his *friend*, for a man of his own race and blood!" said a young Greenlander. "But these Englishmen are strangers to us." "Let us share what we have with them. Hunting may improve", said one of the women, hopefully. "There will be no good hunting this winter", replied an old hunter.

And so they talked on, and eventually all were agreed as to what it was best to do. And on the third day the Englishmen came to hear their decision.

The questions in this instance are:

(1) What did the Greenlanders decide to do?

(2) What do you think it will be best to do, and *why* will it be best?

(3) Do you think Greenlanders and Europeans can understand each other and be real friends?

This is by far the longest story used in Greenland. It was written mainly in order to see whether any real attempt would be made to sift a very complex set of data. The risk of including some rather critical statements about Greenlanders was taken because it was thought that, in such a setting, they would not be likely to cause offence, and might even encourage a free and genuine expression of opinion.

(ii) *Method of presentation*

This story was used in Umanak only, with a group of thirty-four subjects—sixteen men and eighteen women. The method of group presentation described above (p. 298) was adopted.

(iii) *Analysis of results*

Tables VII and VIII show the kind of answer received to questions 1 and 2, and the relative emphasis on each.

Table VII

What did the Greenlanders decide to do?	Men (16)	Women (18)	Total (34)
To help the Englishmen	15	17	32
Doubtful	1	1	2

Table VIII

What do *you* think it will be best to do, and *why*?	Men (16)	Women (18)	Total (34)
(*a*) To be friends and give mutual help	13	16	29
The Greenlanders ought not to help *too much*	1	—	1
No answer	2	2	4
(*b*) The Greenlanders must help, because they have received help	9	13	22
For the sake of the good name of all Greenlanders	—	2	2
No answer	7	3	10

No one predicts that the Greenlanders will *not* help, though some say that "they will give only what they have to spare, and the Englishmen will understand that". Only two subjects, both old hunters, refer to the arguments in the text *against* helping, and they merely dismiss them without discussion as "stupid and wrong". Question 1, in fact, was answered with the same speed and confidence as was apparent in subjects' reactions to Examples I and II, and practically all answers seem to have been immediately and completely dominated and directed by the sentence, "we ought to help them, because they helped us", for there is an almost monotonous recurrence of the statement, "The Greenlanders must help the Englishmen, because they helped them", along with an apparent blindness to obvious practical difficulties, and the force of any other argument whatsoever. One can only speculate, of course, as to *why* this phrase should be selected and dominant, but at least part of the reason must be that this particular story succeeded in provoking the same 'group self-consciousness' and 'group pride' as were apparent in some subjects' reactions to Example I (p. 300). This emotional element, aroused evidently by the preliminary argument about the giving of gifts and the whole trend of the narrative, is reflected in the tone and phrasing of numerous generalizations, e.g. "Greenlanders are more backward in their way of thinking than other folk, but they gladly give something on all occasions"; "Greenlanders are exceedingly helpful"; "we Greenlanders are in the habit of repaying what has been done to us"; "we Greenlanders have a custom from old times of helping one another."

Question 2 was put in order to see whether any subjects would differentiate between what *would* be done and what *ought* to be done. As one would expect from the general attitude adopted towards question 1, and the type of answer which it elicited, no such distinction is drawn. The unusual hesitancy which was shown in dealing with question 2 can therefore only be attributed to the fact that most subjects could see no real difference between it and question 1, and were wondering why they had been asked to repeat themselves.

While the men were nearly all agreed that it is best to help, they are more reluctant than the women to say *why* they think so. The only explicit justification of giving help is a repetition of what was said in reply to question 1: "The Greenlanders must help, because they have received help". To this two women add, "and also for our own sake", meaning evidently, "for the sake of the good name of all Greenlanders".

This kind of 'moral justification' is interesting in itself, but the fact that no attempt is made to consider in detail practical working difficulties is rather disappointing. The only suggestion of this kind was made in conversation by a Dane, who said that the Greenlanders would probably help the Englishmen, but in dividing the spoil would give priority to their own hunters, who at all costs must be sufficiently nourished to hunt. This was the old Eskimo custom, and perhaps might be referred to if ever this material is used on some future occasion in isolated hunting communities.

Question 3 was answered as shown in Table IX.

Table IX

Do you think Greenlanders and Europeans can understand each other and be real friends?	Men (16)	Women (18)	Total (34)
Yes. Both	9	16	25
Good friends, but no real understanding	3	—	3
Understanding, but no real liking	1	1	2
No. Neither	1	—	1
Doubtful	2	1	3

The women are much readier than the men to give an unqualified 'yes'. This is further emphasized by a fact not shown in Table IX, that five of the nine men make this reservation—that Greenlanders and Europeans will only understand each other and be real friends *if* they live together for a long time, and the education of the Greenlanders improves. Two of the women mention the former of these two conditions, and two others, that it will be necessary to learn a common language. An interesting expression of group attitude is found in the statement, made by two subjects (a man and a woman), that real understanding and real friendship are possible "in spite of the great difference of wealth, and although Europeans are wiser". On the other hand, while the three men who think that Greenlanders and Europeans can only *like* each other, acknowledge this "difference in intelligence", they find it, and also "difference in custom", an insuperable obstacle to real understanding.

(iv) *General conclusions from Example III*

On presentation of this material, one item was immediately picked out and so heavily 'weighted' as to be completely dominant.

Reasons why this should be so have been suggested (p. 307, and cf. Example I, p. 300), and its effects are, as in Example I, the production of easy, stereotyped answers and the evoking of a distinctive type of generalization.

The ease with which question 1 was answered, as compared with question 2, the failure to differentiate between them, and the marked absence of any discussion of practical difficulties, all indicate a tendency on the part of most subjects to identify what would happen with what they should wish to happen.

Question 3 will be considered later in a general discussion of the secondary aim of these experiments.

VII. EXAMPLE IV

(i) *Material*

The Haunch of Venison

It was unfortunate for Johan that the dogs should have passed at that moment. You see, Johan had been working for the outpost trader for nearly six months, and on the whole he and the trader understood each other fairly well. Johan was capable, and the *bestyrer*,[1] who was a careful man himself, liked him for that, and also because he was clean and quiet. The trader, like most men who talk a good deal, was rather hot-tempered, but as Johan knew, his bark was worse than his bite, and he was really rather a kind-hearted man. So Johan kept out of his way when he was in a bad mood, and generally the two of them got on very well together. But Johan had one fault. He himself could not see anything wrong in what he did, so perhaps one cannot blame him too much. After all, the trader had more than he required of most things, and if Johan *did* help himself now and again to a little food, a little coffee or a few cigarettes —well, as long as the trader knew nothing about it, then he was no less happy and Johan was a good deal happier.

At the end of his fourth month, and again during his fifth, the trader told Johan in no uncertain language what he would do to anyone that he caught pilfering. He did not accuse Johan directly, but Johan was wise enough to take the hint, and mend his ways.

And that brings us back to the dogs. Johan was one day taking a haunch of venison out to the little cold store-house where the trader kept his meat. Johan happened to forget the key of this little house, which was always kept locked. The venison being heavy, he hung it on the handle of the door and ran back quickly for the key. Just then the dogs came round the corner, and by the time Johan got back there wasn't much venison left. First of all Johan thought it would be best to tell the trader what had happened; but then he remembered that the trader was busy, and in a bad mood. So Johan said nothing about the accident, and hoped that the trader would forget his venison.

A few days later, however, the trader asked for his venison, and Johan had to tell him how he had lost it. Poor Johan did not tell his story very well, or very convincingly, for he saw from the very beginning that the trader was not believing him. When the story was finished, the trader laughed—a short hard laugh—and then he looked at Johan and said: "You are the only man other than myself who is allowed to use the key to my storehouse. I don't think it was the *dogs* who ate the venison." He laughed again, but only with his mouth, and Johan could see the anger growing in his eyes.

[1] Trader.

The questions in this instance are:

(1) What will the trader do?

(2) What will Johan do?

(3) With whom do you sympathize more, and why?

(ii) *Method of presentation*

This material was used in Umanak only, in a group experiment with thirty-three subjects—sixteen men and seventeen women. The method of group presentation described above (p. 298) was adopted.

From the general type of reaction observed, it seems that this attempt to write a story which would be taken light-heartedly was fairly successful. All laughed expectantly at the title, as if it gave promise of something amusing to follow. There was little or no reaction, amused or otherwise, to the account of Johan's pilfering,[1] but the loss of the venison, and Johan's subsequent behaviour, up to the telling of his story to the trader, caused hearty and prolonged laughter. The trader's reply, on the other hand, was not considered funny, but had a marked sobering effect on the group as a whole.

(iii) *Analysis of results*

Tables X–XII show the kind of answer received to questions 1–3 respectively. Questions 1 and 3 were answered promptly; question 2 much more deliberately.

Table X

What will the trader do?	Men (16)	Women (17)	Total (33)
Dismiss Johan	5	3	8
Be extremely angry and scold Johan	7	12	19
Be extremely angry, and then pity, understand and pardon Johan	3	2	5
So angry at losing his venison that he will not eat for a long time!	1	—	1

Table XI

What will Johan do?	Men (16)	Women (17)	Total (33)
Accept his dismissal and/or give up his job	5	10	15
Give up his job, but be taken back	1	—	1
Explain, apologize	10	5	15
Become even more zealous	—	2	2

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[1] This kind of petty pilfering and the fact that many Greenlanders condone it, even if they do not practise it, is said to be a relic of the old Eskimo communism. The Greenlanders know very well how much the Danes detest it.

Table XII

With whom do you sympathize more?	Men (16)	Women (17)	Total (33)
Johan	14	16	30
Trader	1	—	1
Neither	1	1	2

Although the total number of subjects is small, the following comparative percentages are interesting, and seem to indicate significant differences of opinion. Taking all the answers together, approximately 54% predict only a short burst of anger on the part of the trader; 25% are certain that he will sack Johan; and 15% think that he will be reasonable, and compassionate. Opinion is almost equally divided that Johan will either give up his job or explain, apologize and presumably continue work.

Taking men and women separately, however, it is clear that the women are more inclined than the men to predict a short burst of anger on the part of the trader, and rather less inclined to think that he will sack Johan; but nearly 60% of them think that Johan will *voluntarily* give up his job, and only 30% that he will try to explain. In the case of the men, these last two percentages are exactly reversed.

The opinion expressed by the women is in keeping with what the Danes say of their Greenland *women* servants—that you can scarcely scold them for fear they run away! In this instance, therefore, the women's answers may have been largely determined by their imputing to Johan conduct more likely to be expected of a native woman. The opinion expressed by the men, on the other hand, is in keeping with the treatment they are accustomed to either as casual or regular workers under the Danes. Even in the coal-mine and marble quarries, when they are in regular employment for wages, conditions make it inconvenient to have them idle, and a kindly administration has so far discouraged any attempt to introduce strict working regulations, and the threat of dismissal and unemployment. Greenland men, therefore, have little experience or fear of being sacked, and perhaps that is why the majority of men subjects in this experiment seem confident that a full explanation and apology will be effective.

Sympathy is overwhelmingly in favour of Johan: "because he'd been unlucky in losing the venison, and in being falsely accused" (fifteen subjects); and "because he was afraid and couldn't tell his story properly" (four subjects). Four other subjects pity him for his faults; "he'd committed crimes"; "he couldn't go straight"; "he was foolish and negligent"! The one man who liked neither the trader nor Johan thinks that the former is "hot-heated and stingy" and the latter "dishonest"; while the humourist who predicts that the trader will not eat for a long time out of sheer annoyance, sympathizes with him greatly, but thinks that "he'll duly recover his appetite and have Johan back"!

(iv) *General conclusions from Example IV*

The general types of answer to question 1 are so much in keeping with the description of the trader's character in the text that it seems reasonable to infer that this description was heavily weighted by most subjects, though no one of these makes explicit reference to it. In other words, subjects seem in this instance to have utilized specific data presented in the text, and not to have relied to any appreciable extent on socially accepted generalizations concerning white men and their behaviour.

In answering question 2, on the other hand, both men and women subjects go outside the limits of the data presented, the men predicting that Johan will behave as most Greenland *men* would in similar circumstances, and the women that he will behave as most Greenland *women* would.

Although no explicit generalizations are made, the answers (especially to question 2) seem to reflect definite and fairly widely accepted opinions concerning the reciprocal relationship of Danes and Greenlanders, and its expression in practice.

VIII. RESULTS

(i) *The nature of constructive thinking*

In preliminary and tentative experiments of this kind one is more concerned with the value and possibilities of the method employed than with reaching immediately any very definite conclusions. None the less, the four experiments described, and others conducted last summer in Greenland, have served to draw attention to certain features which seem characteristic of all subjects' reactions to this type of problem situation. These characteristic features are as follows:

(i) A quick confident response.
(ii) Easy, stereotyped answers.
(iii) Either a high percentage of agreement, or disagreement along two or more very definite lines.
(iv) The frequent formulation of generalizations of a very distinctive type.

(v) A constant tendency to pick out one item, or set of items, to which completely dominating weight is given, and very rarely any attempt to utilize all the data presented.

(vi) A tendency to identify what *will* happen with what 'must', 'ought', or 'should' happen.

(vii) Consequent upon (v) and (vi), a tendency to make the facts fit the solution, rather than vice versa.

(viii) In (iv), (v), (vi) and (vii), considerable evidence of emotional involvement.

What the experiments demonstrate most clearly is that constructive thinking about problem situations of this type is highly 'conventionalized". This is neither an unexpected nor a novel conclusion, and any attempt at this stage further to define the precise nature of this 'conventionalization' would be speculative and premature. But the manner in which the fact of conventionalization has been demonstrated serves to direct attention to another fact which admits of direct investigation by objective methods, viz. the fact of *selection*. By the comparative study of social groups within one culture and of different cultural groups, it should be possible to obtain by more carefully controlled experiments valuable evidence as to how this process of selection operates, and some indication of the relation in which it stands to conventionalized patterns of thinking.

One quite definite result can, however, be immediately stated. A series of experiments, similar to those described, but using problem situations constructed to suit a different social background, has been carried out in Cambridge by Prof. F. C. Bartlett.[1] A comparison of notes and results proves at once that the kind of reaction observed in England, and the type of answer received, display precisely the same general characteristics as those described above. It therefore seems reasonable to infer that the mental processes involved in dealing with this kind of problem, at least, are, in each instance, *psychologically* identical, and that the contention that civilized persons strive always to reach a well-balanced coherent conclusion, while the primitive is willing to tolerate incoherence and even 'considerable contradictions and contrasts' is, if not unfair to the primitive, at least highly flattering to ourselves.

(ii) *The method*

(*a*) *In relation to the primary aim of the experiments.* From the examples given, it will be seen that the method possesses the advantages claimed for it above (p. 297), and also, of course, that it can be extended and improved. As the tendency to conventionalize, and to go beyond the limits of the data presented, must be at least partly due to the type of 'incomplete situation' employed, the first step is to devise and try out 'control situations' which can be solved, to a greater or less degree, within their own limits.

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[1] The results of these have not yet been fully published.

(*b*) *In relation to the secondary aim of the experiments.* As an enquiry by this method into the attitude of Greenlanders to Europeans would require, in order to yield results of any value whatsoever, a much more extended survey than was possible last summer, discussion of the secondary aim of the experiments is necessarily confined to a few notes on the use of this method with primitive people.

As compared with the questionnaire, this method has the following advantages:

(1) Nearly all primitive people, and certainly all Greenlanders, in response to a direct question tend to give an answer which they think will please the questioner. In all the experiments conducted in Greenland, no evidence of this kind of insincerity appears, and there is considerable evidence pointing in most instances to genuine and sincere answers.

(2) Insincerity seems to have been eliminated, and sincerity encouraged by (*a*) the general group attitude to *any* story, and (*b*) the specific emotional 'atmosphere' created by the actual kind of story used.

(3) Within the context of a well-chosen story, points which might otherwise cause offence can be safely raised.

(4) By referring them to an actual concrete situation, the questioner can be reasonably sure that his questions have the same essential meaning for his subjects as they have for himself.

(5) If we ask an individual a set of isolated questions concerning group relationships, then we can scarcely hope to know what aspects of his experience he has recourse to in framing his answers. If, however, we describe a situation in which certain given factors are *possibly* relevant, then we can observe what selection and weighting of these factors occur, and what further factors, if any, are introduced. In other words, we are enabled partially to control our subjects' 'frames of reference'.

Too much indirectness, on the other hand, is not desirable. Thus in Example II (Tables V and VI) and again in Example IV, Table XII, we have sets of answers which *probably* reflect fairly definite and widely accepted opinions concerning the reciprocal relationship of white men and Greenlanders, and its expression in practice, but which leave the exact nature of those opinions more or less a matter of speculation. Although it is doubtful whether Greenlanders (or indeed *any* social groups) could give full and precise *verbal* expression to such opinions, none the less a partial expression might be obtained by fairly direct questions following upon the presentation of a relevant situation (such as question 3, Example III), and by employing the method of individual presentation and oral response (Example I) which, as we saw, gave by far the richest crop of significant generalizations.

(iii) The method is practically self-maintaining, producing unsolicited generalizations which, if thought significant, can be embodied in further specially devised situations. The study of the nature, frequency and distribution of the conventionalized opinion of which these generalizations are a partial expression can thus be extended indefinitely.

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THE CONCEPT OF THE 'SCHEMA'.[1]

By MARY L. NORTHWAY

From the University of Toronto

PART I

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II. *The concept of the 'schema' and its manifold uses* (pp. 317–318).

III. *The immediate background from which Bartlett's theory developed* (pp. 318–323).

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  (i) *The influence of the 'difficulty' of the material on remembering* (pp. 323–324)

  (ii) *The influence of age and social group on children's remembering* (pp. 324–325).

I. INTRODUCTION

THE purpose of the experiments herein reported was to examine the concept of the 'schema' used by Bartlett in his theory of remembering.[2] The various meanings he gives the term and the sources from which it developed are examined. Investigations in primary schools pointed to an interpretation which appeared to be sufficiently definite to direct research, and which serves to indicate some educational implications of Bartlett's point of view.

The work of Bartlett has had special significance in two ways. First, it has demonstrated that a developmental psychology, of the type propounded by Ward, may be subjected to rigid experimental investigation. Secondly, it has reaffirmed the notion that memory is no passive storehouse in which items are retained and arranged according to certain mechanical principles, but rather is that process by which the active individual recreates his past experience in ever-evolving forms. This dynamic process, remembering, is influenced by the individual's "appetites, interests, ideals and social background"; and of particular pertinence in the organization and re-organization of experience is the notion which Bartlett has termed the 'schema'.

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[1] Abstract of a thesis accepted by the University of Toronto for the Degree of Doctor of Philosophy.

[2] References, unless otherwise indicated, refer to Bartlett's *Remembering* (1). All references will be given at the end of Part II of this paper, to be published in a subsequent number of the *British Journal of Psychology*.

II. THE CONCEPT OF THE 'SCHEMA' AND ITS MANIFOLD USES

In the study of remembering as an active process Bartlett attempts to give the concept of the schema an important place. Exactly what this place is, is difficult to define, not because the term is used vaguely, but because it is used in so many different ways. And because one use is interchanged with another without any explanation, a great deal of confusion arises.

Four different uses of the term are listed below:

(1) The schemata are considered to be the *forces* within the individual which determine what the subject's reconstruction will be:

> Determination by schemata is the most fundamental of all ways by which we can be influenced by reactions and experiences which occurred sometime in the past (p. 201).
>
> What, precisely, does the 'schema' do? Together with the preceding incoming impulse it renders a specific reaction possible. It is, therefore, producing an orientation of the organism towards whatever it is directed to at the moment. But that orientation must be dominated by the immediately preceding reaction or experience. To break away from that the 'schema' must become not merely something that works the organism, but something with which the organism can work. The organism discovers how to turn around on its own 'schemata'. In other words, it becomes conscious (p. 207).

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(2) The schemata are the *forms* in which the individual preserves material.

> It looks as if that preservation of material which is required in recognizing is normally a preservation of schemes, of general settings, of order or form of arrangement (p. 195).

(3) The schemata are 'storehouses' in which *content* is retained while it is being reorganized.

> In fact this is one of the great functions of images in mental life: to pick items out of the 'schemata', and rid the organism of over-determination by the last preceding member of a given series (p. 209).
>
> All of us in reference to some of our 'schemata' have probably completed the model and now merely maintain it by repetition (p. 203).

(4) 'Schema' is used in a way approximating the notion of the *apperceptive mass*.

> I think probably the term 'organized setting' approximates most closely to the notion required (p. 201).
>
> The influence of the 'schemata' is influence by the past. In its schematic form the past operates en masse, or, not strictly en masse because the latest incoming constituents which go to build up a 'schema' have a predominant influence (p. 202).

Although this analysis separates the various meanings of the term artificially and implies they are more distinct, one from another, than they actually are in Bartlett's use of them, the confusion resulting from using so many conflicting meanings is apparent. It should be noted that although Bartlett states that he is considering remembering as an active re-organizing process, in many of the cases he gives, the idea of memory as a 'storehouse'—as content carried forward—seeps in all too easily. In order to clarify and appreciate the various connotations given the term, the first piece of research was to trace the sources from which it came. This consisted of a study of the immediate background from which Bartlett's work developed.

III. THE IMMEDIATE BACKGROUND FROM WHICH BARTLETT'S THEORY DEVELOPED

The problem of this section is to select some of the influences which have directed Bartlett's psychology and his concept of 'schemata' as he uses it in his theory of remembering, with the purpose of using this information to clarify the meanings he gives to the concept. While the notion could be traced back almost endlessly, this section is limited to the discussion of its immediate background.

If the title of Bartlett's book, *Remembering: A Study in Experimental and Social Psychology*, is analysed, the three main influences on his thought are revealed. The 'experimental' aspect is closely associated with the work of Sir Henry Head and the physiological group; the 'social' aspect is related to the influence of Rivers and the Cambridge anthropological interest, which developed from the days of the Torres Straits expedition; and the 'psychology' is found to be in direct descent from that of James Ward, the most influential of Cambridge teachers in Bartlett's time.

Although these three influences have been isolated for discussion, in order to appreciate Bartlett's psychology it is necessary to bear in mind the close interrelationships which exist between various fields of study in the Cambridge setting. Demarcation between fields of research, and departmentalizing of subjects, are far less rigid than in Universities on the American continent. Facts, theories and investigations from the various sciences are absorbed together, and a psychology to endure must not only be consistent in its own right, but must not conflict with findings recorded by the physiologist and sociologist. Bartlett's knowledge of, and keen interest in, these fields, and his own background in the 'moral sciences', influence very greatly the psychology he derives, and may be responsible not only for the breadth of his point of view but for some of the confusions which arise. This is particularly true with regard to the notion of schemata, which Bartlett derives from psychology proper, from physiology and from anthropology; and it is in attempting to form a concept which is in harmony with thought in all these fields that a great many of the difficulties arise.

(*a*) *Sir Henry Head.* In order to appreciate Head's influence on Bartlett the fact that a relationship more personal than purely academic existed between the two men should be taken into account. Each in the introduction to his major work (1), (8) acknowledges his debt to the other, and Bartlett in a critical notice of Head's two volumes on Aphasia writes of it in eulogistic terms which show a close sympathy for its development and implications.

> For the psychologist these are two of the most important books which have appeared for many years. They are scientifically and aesthetically satisfying....I believe these books will have a permanently important place...in the history of shaping psychology into a science...the general viewpoint will remain of abiding interest and lasting importance. And as with all genuinely valuable achievement, Dr Head's life work and particularly this crowning effort will be a most powerful stimulus for further research (2).

Bartlett, being so much impressed with Head's work, felt very strongly that his own must not only be in harmony with it, but also indicate the close relationship by using (in modified form) one of the terms which Head had made so significant in recent biological thought. This term was 'schema'. Head defined it as:

> That combined standard against which all subsequent changes in posture are registered before they enter consciousness (8).

Although Bartlett in taking this term over from Head claims his right as a psychologist to modify it to mean "an active organization of past reactions" (p. 201), the above definition, with its emphasis upon a 'standard', contains the idea which appears, here and there, in Bartlett's work—that the schema represents the combined *content* of previous reactions heaped up like the *apperceptive mass* and taking on a certain *form.* These three uses of the term have at least one of their sources in the influence of Sir Henry Head. The failure of Bartlett to retain the modification of the schema which he has stated, and his regression to its use as a somewhat static standard, constitute one reason for confusion in its meaning; the unsuccessful attempt to reconcile this concept of a standard with a Wardian psychology is another.

(*b*) *James Ward.* The principles of Ward's psychology are implicit throughout the work of Bartlett, who was not only his pupil but who maintained a close relationship both academic and personal with him for many years. Ward's psychology has been summarized by Brett in this way:

> The unity of consciousness is a continuance; the whole process must be regarded as the gradual differentiation of a primary unity...the mind is an organism which grows by continuous development and differentiation into a collection of parts that develop in sustained relations and never change without involving reciprocal change through the whole structure (6).

These ideas of continuance and development are assimilated so intimately by Bartlett that he assumes them throughout his writing without making any explicit reference to them. It is in a psychology based on these that schemata in remembering have to be made meaningful.

While Bartlett's general point of view is based on Ward's psychology, Ward had also a specific influence on Bartlett's theory of remembering. Writing an obituary of Ward in the *American Journal*, Bartlett states:

> Another part of psychology had an irresistible attraction for him, the study of memory. Here if anywhere was to be found actual evidence of the integrity and independence of mind, and the indissoluble character of individual experience (3).

Ward, who himself emphasized the influence of Lotze in his thought (3), acted as a direct bearer of the idea of the activity of memory from Lotze to Bartlett. This legacy included with it the concept of the schema thought of as a *plan* of activity, the *form* which the activity will take. Brett writes:

> To Lotze and all idealistic psychologists the memory is the crucial instance; there is no faculty of memory, no cerebral organ, no 'storehouse of ideas'; only the living continuity of the soul and its power of reproducing its previous activities....Memory does not keep any picture; what it really retains is a kind of *schema*, a plan of action (6).

This is the meaning schema has in the psychology Bartlett assumes, but it is a meaning he does not make explicit or develop. It should be kept in mind as a possible interpretation of the term when it is used in a theory which considers remembering as an active process belonging to a 'developmental' psychology. This use will be referred to in the experimental section of this paper.

The most important influence Ward has on Bartlett's theory of remembering, however, is the significance he gives to interest. Ward casts aside association as an explanatory principle and substitutes continuity for contiguity (11). The continuity of the individual's activity is considered the integrating principle. In answer to the question of how new wholes are integrated, Ward restates the Lotzian point of view "that movements of attention determine what wholes will be formed and these are based primarily on *interest*" (6). Ward does not think of items of content being absorbed and then integrated by some superimposed process of association, but rather, he considers the activity of the individual through his interest to be in its very nature integrating and developing.

This function of interest is taken over and becomes important in Bartlett's theory. The schemata are built up by several processes in human life; but the chief of these are the interests which sift and evaluate the 'elements' of the schemes.

> In all these 'schemes' there is operating that kind of unwitting analysis which gives weight to certain elements of the whole....Most often of all, in human adult reaction it is a matter of the operation of persistent 'interests' (p. 302).

Here again, while attempting to think in Ward's terms of activity, the contradictory concept of memory *content* creeps in.

Bartlett's basic psychology is essentially Wardian; and his theory of remembering with its emphasis on interests is in harmony with this psychology. It is paradoxical that while this psychology is assumed by Bartlett, the use of schema which is consistent with it—the schema being a plan of the activity rather than a form into which content is put, or a force which causes the activity—is not developed in his theory. Instead, he attempts to substitute in his Wardian psychology the idea of schema as a mass of content which the interests serve to organize. The failure to develop the meaning of schema which is consistent with the psychology he develops, and the attempt to make the term harmonious with concepts in other fields such as these of Head and Rivers, is the ultimate basis of the constant confusion.

(c) *W. H. R. Rivers*. Bartlett's familiarity with Rivers's work and his own early and continuing interest in anthropology (4) add more confusion to the 'schema'. From his interest in the persistent group patterns and conventions which mould new activity into their own forms and change these forms very slowly, Bartlett works by analogy to the idea of the function of schema in individual activity.[1]

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[1] That the analogy develops from Bartlett's interest in social processes and is taken over to the individual field is apparent from a study of Bartlett's social psychology—see § 3 of *Remembering*.

> The direction of a group's preferred, persistent tendencies constitutes a kind of lasting social 'schema' on the basis of which much successful constructive work in recall may take place (p. 264).
>
> It may be that social conventions, institutions, and traditions formed by persistent group tendencies constitute 'group schemata'; just as the individual images, ideas and trains of thought formed by persistent interests constitute 'individual schemata' (p. 299).

The schemata here have the meaning which Bartlett retains when he uses the concept to mean the forms which are imposed on new material and the forces which determine what the remembering activity will be.

A second systematic difficulty arises from Bartlett's relationship with sociology. According to his theory of remembering, interests, appetites, instincts and ideals influence remembering as they function in the schemata, but the social background, he considers, influences the individual directly and not as part of any psychological process. Remembering is determined, then, in two ways; first by the schemata, and secondly, and separately, by the social group. Therefore, the schema has to be given a meaning which will harmonize its effect with the effect of the social group. This is one source of the confusion which results when the schemata are thought of as forces.

(*d*) *Bartlett's early studies.* One other influence on the development of the concept should be noted—namely, Bartlett's own early experiments in perception. In an article, published in 1916, on "Some problems of perceiving and imaging" (5), he states that in perception the subject always makes an 'effort after meaning'. This meaning "depended greatly upon the prior experience of a subject and his constant interests or those of the moment". In his book of 1932, in reporting on these early experiments, he says, "I shall call this fundamental process of connecting a given pattern with some setting or *scheme* 'effort after meaning'" (p. 20). In this early notion of the influence of both past experience and interests in forming the scheme, the relationship of the concept with that of the 'apperceptive mass' is seen on one hand, and on the other its relationship with Ward's notion of continuity based on interests.

(*e*) *Summary.* In the above discussion the various influences which affected Bartlett's concept 'schema' have been indicated. These may be summarized briefly here. Although Bartlett's psychology is essentially that of Ward, he does not develop the idea belonging to this psychology of the schema as a 'plan of the activity'. He attempts rather to reconcile his use of the term with physiology on the one hand and anthropology on the other, and is thus led into employing it in several senses. The schema is at times thought of as comparable to the forces exerted by social custom on behaviour, at times as a form which is like that both of a biological standard and of a social pattern, and at times as the content of memory and the apperceptive mass into which new material is received, even as new physiological impulses are registered against the stored effects of previous ones. The confusion which results makes systematic appraisal difficult and experimental development vague. The term itself must be clarified and defined. This is here attempted by further investigation of remembering as an active process; and through this will be determined where and how (if at all), the term best applies.

Accordingly, in the next section we turn to experimental studies of remembering. In these we shall consider at what point the concept of schema is necessary and in what way it may best be defined. Possibly any one of Bartlett's four ways of using the term might be developed, or perhaps some other use of it may be more adequate. The definition we select will be the one which develops from the experimental results.

IV. PURPOSE AND GENERAL METHOD OF THE EXPERIMENTAL INVESTIGATION

The purpose of these experiments is to continue the study of active remembering, and from the results give an interpretation of the notion of 'schema'.

In contrast with Bartlett's experiments with individual adults, these studies have all been made in classroom groups of school-children. The emphasis Bartlett placed on the use of 'real' material, that is, material with which the subject would most likely 'have to deal', has been maintained. This emphasis on 'reality' has been extended in the final experiment to include the use of the actual situation and usual method of learning the material, in so far as this could be done and a satisfactory degree of 'control' secured.

V. PRELIMINARY EXPERIMENTS

(i) *The influence of the 'difficulty' of the material on remembering*[1]

While this particular experiment arose originally as a by-product from another study, the results are recorded here to show a preliminary attempt to develop a definition of 'schema'.

Forty pupils in the fifth grade learned one poem a week for four weeks.

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[1] Full reports of this and the following study have been published in the *British Journal of Psychology* (12, 13).

The learning periods were equal and the poems were all of the same length and form, but differed in the difficulty of their thought content. Four weeks after the last of these had been learned the children were asked to write "what they remembered of each poem, or what story it told". The number of words which were reproduced immediately after a learning period was taken as the objective criterion of the degree of difficulty the poems had for the subjects. With the difference in difficulty a second difference demonstrated itself. This may be considered a difference of "what the subject has made out of the material". With the simplest poem, the subject constructed a recall which, with a few minor changes, was closely determined by the given material. As the material became increasingly difficult, the subject's constructions showed more importation, less close adherence to the original and more individuality.

What place can we give to the notion of schema in this interpretation? There are two differences, one of content achieved, the second, of what the children have made from the material. If this second difference were called a schematic one, then the schema would have the meaning of "what the subject makes from given material". In the easy poems the schemata are closely related to the material; as the poems become more difficult the schemata show more invention and substitution. The use of the word schema to indicate this non-quantitative difference in what "the subject makes of material" is valuable in the interpretation of this experiment. The following experiments investigate whether this use can be maintained, further clarified and extended.

(ii) *The influence of age and social group on children's remembering*

Bartlett stresses the fact that social background influences remembering. In giving the same story to subjects from English and Indian cultures he found certain differences in recall.

> The variations (between the English and Indian subjects) seem to be in accord with group differences. Both the character and operation of group differences could be studied by the careful use of the Method of Serial Reproduction (p. 146).

In this experiment recalls were obtained from groups with a much smaller difference in their social background. The subjects were children from three schools in different districts of Toronto, with as widely different social backgrounds as possible in one community. Three stories, each approximately 250 words in length, but varying in type, style, and difficulty were used. The chain or serial reproduction method (p. 119) with 2–8 chains in each group for each story was used. The subjects read the story through once and immediately wrote what they remembered of it. They then left the room and the subjects for the next link came in and each read one of the former subject's reproductions. A week elapsed between each story. The chain was carried on until either the subjects or the material were all used.

Differences in remembering among the groups were as follows:

(1) In the reports of the older children (14–15 years) more of the given material was retained than in those of the younger children (10 years). The younger ones imported and substituted their own ideas.

(2) The less stable social group gave many more modifications and importations than the comparable age group in the more stabilized social setting.

(3) Items having a high positive value of social meaningfulness were remembered most readily.

These results support the rather obvious claim that social group and age influence remembering. Have they any contribution to make to the clarification of the concept of schema? In considering this we must refer again to our section on the historical development of the notion. There it was pointed out that Bartlett considered remembering to be determined by the schema and, as a result of Rivers's influence, by the social group. This, we mentioned, led to some confusion; for remembering was determined first by a force which was psychological in nature, and secondly, by a force belonging to an anthropological frame of reference, namely the social group. From these results it is suggested this confusion may be clarified in this way. As in our first experiment we are concerned primarily not with quantitative differences, but with differences in "what the subject makes out of the material". These, we suggested, might be called schematic differences. Using the term in this way we need not think of schema and the social group as two separate forces influencing remembering, but rather that social background and age are factors which are reflected in the schemata or "what the subject constructs from the material". The schemata reflect the characteristics of the group as they are incorporated by the individual. The members of the heterogeneous group show more diversity in their schemata than do those of the stabilized group, the younger children show more originality than the older ones. And finally, using the term schema in this sense conveys the significance that age and social group do not influence the quantity of material remembered, but the form the activity takes.

Summarizing the results of these experiments in terms of the use we have assigned to the word 'schema', it may be stated that "difficulty of material, age and social group are all factors which influence the memory schemata".

A NEW APPROACH TO THE TESTING OF MUSICAL ABILITY

By BORIS SEMEONOFF

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I. INTRODUCTORY

TESTS of musical ability may be classified into two main types—those that measure proficiency resulting from education in music, and those that measure some innate capacity for the performance or appreciation of music. Existing test schemes have not always distinguished between these two types, which correspond to definitely different aims. Even some of those purporting to be of the second type have been cast in a form which has demanded knowledge on the part of the testee of musical terminology, or has otherwise placed individuals with musical experience at a distinct advantage. The purpose of the present paper is to suggest the possibility of the use of a new principle in the testing of one aspect of untrained musical ability.

In point of fact, the subjects of the present experiment all had some knowledge of music, however slight, and the material was chosen with this particular group in mind. The group consisted mainly of students of the Ordinary Psychology Class of the University of Edinburgh, but a few other people were included, all with a similar educational background. In addition, use was made of a small group of students in the Faculty of Music, who acted mainly as a control. It is necessary, therefore, to stress the fact that this paper describes an *approach* to the testing of musical ability. It was hoped that if the method proved successful with the student group, it might also be applicable to school children, down to quite an early age.

II. PRELIMINARY EXPERIMENTS

The method of testing used evolved gradually. Some hints were obtained from Schoen (3). Since many of the researches described in that book seemed to be lacking in general conclusions, two forms of questionnaire were drawn up, and tried out in consecutive terms with the Ordinary Psychology Class during session 1937–8.

In each case a series of gramophone records of music of various types was played, and the hearers reported their replies to certain questions. A good portable gramophone was used, and, except in a few cases, non-metallic needles were preferred to steel. It was thought that the variety of music available on gramophone records, and the assurance of constancy of performance, more than compensated for the lack of 'human element', since the writer's personal opinion was that such 'human element' might be prejudicial to one's judgement.

The form of questionnaire first used was as follows:

Name.. Date.....................
Do you sing, or play any musical instrument?.....................
Do you attend concerts? If so, of what kind?..................

After each selection is played, answer the following questions: (*a*) Did you like it? (*b*) Can you identify it (guess if you like)? (*c*) What impression do you think the composer is trying to convey? (*d*) Did the music give you any emotional experience?

Spaces for replies followed.

An examination of the results led to the following conclusions:

(i) A large proportion of the listeners reported that a piece of music was vaguely familiar, but that they "couldn't place it", or "could never remember the name of a tune". This suggested that one could be reasonably sure that even not totally unfamiliar music would be identified by only a small number of hearers.

(ii) The variety of 'impressions' reported in answer to question (*b*) suggested that this diversity might be used either as a basis of classification, or as an indication of understanding on the part of the listener.

(iii) It appeared that the emotional element, at least under test circumstances, was considerably less important than had been expected, and normally did not prevent the subject from responding in the other ways required.

The experiment was repeated three months later, using different records, and a modified form of questionnaire, as follows:

Name...................................... Date........................
Do you sing, or play any musical instrument?..................
Are you fond of music?...............
What kinds do you prefer? ....

After each selection is played, answer the following questions: (*a*) Did you like it? (*b*) Have you heard it before? (*c*) If so, can you identify it? If you wish to guess, do so. (*d*) What did you think about while the music was being played?

Spaces for replies followed, as before.

Responses to the second form of questionnaire confirmed the first of the conclusions from the previous experiment. Analysis of the replies to question (*d*) of this series further indicated that most listeners spontaneously read a meaning into the music they heard. These 'meanings' for the most part took the form of 'personal' (i.e. egocentric) or what might be called 'general' associations. These latter were considerably the more frequent, especially among men. In all, 420 'general' association responses were recorded, as against 259 'personal' associations. These figures do not include cases of imagery, impulse, emotional experience, like or dislike, or remarks concerning the style or execution of the music, all of which occurred much less frequently. The element of 'meaning' of a generalized character thus seemed to play a considerable part in the experience of the listeners. Finally, a cursory examination of the general questions at the head of each questionnaire suggested that the ability to recognize correctly the composer's intention or other implications of a musical composition depended on the hearer's musical ability, or at least his musical 'background'. The general questions referred to of course explored only the degree to which the subject was *in contact with* music, and gave no indication of actual ability. Other things being equal, it would appear that the musically gifted person would also be interested in and acquainted with music, but of course other things are seldom equal, and it may be that many people with a potential ability to produce or appreciate music are prevented from doing so as a result of lack of opportunity or unsuitable musical education. This view was borne out in the present case by examining the results of an experiment similar to those reported, performed with a small group of students of an Adult Education class in Psychology.

III. EXPERIMENTAL METHOD AND RESULTS

(1) *Basic assumptions*

The theoretical basis of the main study, to be discussed presently, is therefore as follows:

Musical ability implies something more than simply the ability to sing or play an instrument, or to discriminate between simple or complex sensory impressions such as form the raw material of music, and the basis of certain existing tests. It is a widely accepted fact that what distinguishes the really first-rate musician from the ordinary competent orchestral performer is not so much superior technique as interpretative power; similarly, a person with next to no ability as an executant may be a highly gifted critic or even conductor. (In addition it is significant that some of the greatest composers have been indifferent executants.) Interpretative power, then, may be regarded as an important part of musical ability, and this, to the writer's knowledge, has never been separately tested, at any rate on systematic lines.[1]

The question next arises how far one is justified in expecting uniformity of interpretation, and so to what extent one can apply the predicates 'right' and 'wrong' to an interpretation. This, however, is a difficulty which need not invalidate the method of testing employed, although it does limit to some extent the choice of material.

(2) *General method of testing employed*

The method used was to ask the hearer to select from a group of alternatives that interpretation which he thought best fitted the music, this limitation of response being a principle widely used in intelligence testing and elsewhere. Although it might appear at first sight that only music with a definite 'programme' could be used, this is not necessarily the case. If one accepts the broadest implications of Howes's contention (1) that "all music of any value is at bottom programme music, that is, it is a commentary on life, and not something divorced from life", then any musical work in respect of which the composer's intention is definitely known may be used as test material. It may, of course, be objected that the composer may have failed to give adequate expression to his intention, but this is hardly a valid objection if one limits oneself to music on which the consensus of competent opinion shows complete agreement. An experimental check on this point is described later (§ IV (1)).

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[1] Since writing the above, however, I have heard papers, at present unpublished, describing test-batteries including tests of interpretative ability, using methods similar in some respects to that of the present experiment.

The musical material used was selected in accordance with these assumptions, the choice being limited to such gramophone records as were readily accessible to the writer. Ten records was found to be a convenient number to constitute a series, this being the same number as was used in the preliminary experiments. It was possible to play these, allowing for explanation, pauses, etc., in just under an hour.

(3) *A measure of musical knowledge: revised Kwalwasser test*

It was, of course, necessary to use some sort of criterion of musical ability in evaluating the results obtained. Unfortunately, it was not possible to make use of the Seashore Measures of Musical Talent (4) or any other recognized tests, but in any case considerable doubt was entertained regarding their validity for the present purpose. It was possible, however, to obtain an indication of the 'musical sophistication' of the subjects by means of a questionnaire. A fairly suitable test was available in the Kwalwasser Test of Music Information and Appreciation (2), but this was rather long for the time at the writer's disposal, and it also seemed possible to improve upon it in other respects. Accordingly, a radically revised abridgement of the Kwalwasser test was made. The relation between this revised form (RK) and the original (Kw) may be seen in Table I. The last column of figures in the table gives the number of questions not contained in the original form of the test.

The general plan of the revision was (*a*) to omit as far as possible questions of a more intimately personal character, e.g. "Clara Wieck became the wife of Schumann", and those in which 'general knowledge' might be said to play a large part, e.g. proper names of which the form revealed the nationality; (*b*) to include a few questions dealing with vocal music, and modern and light music, not excluding jazz. The Kwalwasser test ignores these almost entirely.

Kwalwasser's Test 1 was omitted because many of the names of performers were unlikely to be familiar to European subjects, or had ceased to be prominent. Test 3 requires uncontrolled naming of composers, which was considered to be contrary to the general plan of the test method used. Test IV of the revised form, devised by the present writer as a substitute for Test 3, was constructed on the multiple-answer plan, and tested detailed knowledge of well-known compositions. In additions to these alterations, minor changes were made in the directions for Tests 2, 5, and 6, and in the examples given.

A correlation of 0·83 ± 0·019 was obtained between the 'retained' and the 'new' items.

The test booklet was provided with a title-page (referred to herein as 'page 1'), which was designed to measure actual contact with music by means of replies to a series of questions. Its purpose was to detect those individuals whose interest in music was of a more active and possibly non-technical kind, but naturally it was difficult to interpret these replies objectively. Thus, for example, in reply to a question "Do you play any musical instrument?" those who say they play 'a little' frequently play considerably better than many who reply simply 'yes'. Correlations between a crude numerical measure based on 'page 1' and various other variables are noted later (§ III (7)).

Table I. *Relation between original and revised forms of Kwalwasser Test of Music Information and Appreciation*

Test no.			No. of items		
Kw	RK	Title of test	Kw	RK	New
1		Classification of artists	20	—	—
2	I	Nationality of composers	20	10	4
3		Composers of famous compositions	30	—	—
4	II	Classification of composers	10	5	2
5	III	General knowledge of composers and compositions	50	15	7
	IV	—	—	12	12
6	V	Production of tone on orchestral instruments	10	5	0
7	VI	Classification of orchestral instruments	10	6	1
8	VII	General knowledge of instrumentation	50	12	2
9	VIII	General knowledge of musical structure and form	50	15	5
		Total	250	80	33

Both 'page 1' and the actual results of the revised Kwalwasser test are thus a little difficult to interpret. Any conclusions drawn depend to some degree on considerations such as to what extent potential musical ability overcomes obstacles put in the way of its development, and, conversely, to what extent training in or theoretical knowledge of music may compensate for lack of original ability. However, acting on the assumption that an individual who had gone far enough to undergo professional training in music would be almost certain to possess genuine ability as well as interest and knowledge, the writer decided to ask permission from the Edinburgh University Faculty of Music to try out the tests with students of the Interpretation Class. A group of twenty to thirty students was made available, and although the number was small, it acted as a useful control group.

(4) *Test material and results: Series A*

Originally it was not intended to draw up more than one series of test records. Accordingly the set referred to as Series A consisted of a first selection of records considered by the writer to be the most suitable of those available, at the same time giving sufficient variety. Series A comprised the following records:

(1) Minuet from *Don Giovanni* (Mozart): harpsichord with orchestral accompaniment.
(2) "By the Waters of Minnetonka": trio—piano, violin and cello.
(3) "Volk ans Gewehr" (modern German song): male chorus with military band accompaniment.
(4) "The Elephant" from *The Carnival of Animals* (Saint-Saëns).
(5) "Finlandia" (Sibelius)—first half.
(6) "Kolj Slavenj" (Bortniansky): Russian church music—Don Cossack Choir.
(7) "Pastorale" (Stravinsky): violin and wind instruments.
(8) Trio in B♭, Op. 99 (Schubert)—1st movement, closing portion.
(9) "La Fille aux Cheveux de Lin" (Debussy): violin arrangement.
(10) "I have attained the power" from *Boris Godunov* (Moussorgsky).

Where no setting is mentioned, it is to be understood that the original form of the composition was used. In the cases of the vocal selections, it was found that in no case were the hearers able to deduce the required answer from an understanding of the words.

The report sheets issued were headed as follows:

Name...................................... Date........................

After each record has been played, answer the following questions: (*a*) Did you like it? (*b*) On what factors did your answer to (*a*) depend? (*c*) Underline the word or phrase completing the description of the music given below.

Although use was also made of the replies to (*a*) and (*b*), the chief interest lay in the replies to question (*c*), samples of which are as follows:

(1) A dance measure, suggestive of (solemnity dignity gaiety abandon).

(10) A soliloquy: the singer's frame of mind is one of (grief despair dissatisfaction defiance).

As will be seen from Table II, these examples represent fairly well the extremes of difficulty involved.

Table II. *Series A: Percentage frequency of responses recorded, of more than one response, and of omissions*

No.	Responses %								Over 1	Omissions
1	solemnity	3	*dignity*	*88*	gaiety	6	abandon	1	4	1
2	moonlight	6	*rippling water*	*73*	bird song	8	trees	3	7	1
3	*determination*	*79*	triumph	10	exhilaration	7	solemnity	—	1	2
4	tiger	15	camel	22	*elephant*	*46*	tortoise	13	—	4
5	sub-tropical	6	temperate and fertile	1	*wild and rugged*	*81*	industrialized	11	1	—
6	resignation	52	thanksgiving	13	communion	24	*praise*	*6*	3	2
7	*pastoral scene*	*28*	serenade	2	eastern bazaar	41	exotic dance	26	1	1
8	austere	2	headstrong	14	carefree	32	*romantic*	*44*	7	3
9	handsome youth	1	*beautiful young girl*	*32*	mature woman	5	gracious old lady	58	—	4
10	grief	27	despair	35	*dissatisfaction*	*15*	defiance	13	6	4

Of the records in Series A, Nos. 1, 2, 3, 6 and 7 had been tried out in the preliminary experiments. Table II shows for each item the percentage of times each of the given responses was recorded, together with, in the last two columns, the percentage of cases in which more than one answer, and no answer, respectively, were given. Correct replies are italicized. The various alternatives should give a fair indication of what each question was. It will be noticed that all possible answers were received, with one exception. These percentages are based on 142 cases—119 members of the Unselected group and 23 of the Musical group. In some cases rather interesting differences between the groups were obtained. These are shown in the first two columns of Table III; the two later columns show similar data for percentages *liking* each selection.

Table III. *Series A: Percentage of Unselected group and of Musical group correct in each selection and liking each selection*

Record no.	Percentage correct		Percentage liking	
	Unselected	Musical	Unselected	Musical
1	95	96	92	78
2	75	58	82	17
3	84	50	61	48
4	37	92	47	96
5	80	87	83	82
6	6	4	65	14
7	25	35	55	64
8	36	58	70	100
9	22	83	68	100
10	19	0	55	50

(5) *Test material and results: Series B*

Since the number of test items was too small to obtain a split reliability, it was thought desirable to try a retest by means of a Series B. Two chief difficulties stood in the way of this: (i) as already indicated, the pick of the writer's records had been used for Series A, and (ii) difficulty was experienced in obtaining again the services of all the subjects who had listened to the first set of records.

The second of these difficulties was partly overcome by asking for volunteers, and eventually, when accidental circumstances had taken their toll, twenty-six subjects remained from the Unselected group, plus a slightly different group from the Interpretation Class. This of course left a rather small number for purposes of correlation, but even so, significant results were obtained, as will be seen. It should also be noted that the 'Unselected' group became rather less unselected, since presumably the students who volunteered were those who had more than the average interest in music.

The other difficulty was also overcome as well as circumstances permitted, and a Series B was drawn up in which each item was matched for type of music, if not for difficulty, with one in Series A. Series B comprised the following records:

(1) "Song of the plains" (modern Russian song): male chorus with band accompaniment.
(2) "The wind in the trees" (Briccialdi): flute.
(3) Polonaise from *Eugen Onegin* (Tchaikovsky).
(4) "Sérénade Espagnole" (Chaminade): violin.
(5) Symphony No. 1 in C maj. (Bizet)—4th movement, first part.
(6) "Tortoises" from *The Carnival of Animals.*
(7) "How beautiful those eyes" (Russian sentimental ballad): mezzo-soprano.
(8) "On hearing the first cuckoo in spring" (Delius)—first part.
(9) "Pimen's monologue" from *Boris Godunov.*
(10) "Poet and Peasant" Overture (Suppé).

Of these, only No. 2 had been tried out previously.

Tables IV and V show the data for Series B corresponding to those for Series A already given. Much less significance can be attached to these, in view of the very much smaller Unselected group which listened to Series B, and of the inferior musical quality of some of the records in this series. It will be noticed that the proportion of omissions is considerably higher; practically all of these came from the 'new' members of the Musical group.

Table IV. *Series B: Percentage frequency of responses recorded, of more than one response, and of omissions*

No.	Responses %								Over 1	Omissions
1	youth organization	13	infantry	4	*cavalry*	*71*	artillery	4	—	9
2	bee's flight	45	cloud of insects	36	rushing water	2	*wind*	*7*	—	9
3	gaiety	52	*stateliness*	*9*	boisterousness	28	grace	—	6	4
4	lullaby	6	*serenade*	*17*	aubade	50	pastoral scene	13	4	11
5	(age)									
	below 20	*31*	c. 30	44	40–45	11	over 60	2	4	7
6	crocodile	22	*tortoise*	*50*	bear	9	elephant	6	—	13
7	joy	2	passion	2	despair	19	*resignation*	*71*	—	7
8	*spring*	*33*	summer	32	autumn	22	winter	6	—	7
9	dejected	50	defiant	2	*old and weary*	*41*	self-confident	—	7	—
10	big city	2	mountainous	15	tropical	—	*rural*	*70*	2	11

Table V. *Series B: Percentages of Unselected group and of Musical group correct in and liking each selection*

Record No.	Percentage correct		Percentage liking	
	Unselected	Musical	Unselected	Musical
1	77	63	77	38
2	8	7	69	3
3	8	11	62	78
4	15	18	85	44
5	23	39	81	85
6	35	64	58	70
7	77	63	58	0
8	4	63	73	93
9	42	39	65	58
10	73	71	85	56

(6) *General results from both series and from the revised Kwalwasser test*

Highest, mean and lowest scores on all measures for the two groups are given in Table VI. The upper line of figures in each represents the Unselected group; the lower line, of italicized figures, the Musical group. Since the numbers tested and the differences obtained are both small, little absolute reliance can be placed on the data contained in this table. Nevertheless, some of the results are sufficiently striking and suggestive to form the basis of the conclusions given below.

The various correlations which were thought to be worth finding are given in Table VII. The figures in the second column are for the whole group, those in the first for the Unselected group only, since in some cases it seemed preferable to consider this group by itself. It will be noticed that some peculiar shifts take place when the Musical group is included. This is particularly so in the case of the correlations between A and B on the one hand, and 'D' and 'E' (see below) on the other. In spite of the considerations noted above, it will be seen that these correlations satisfy the minimum requirements for statistical significance, though considered as reliability coefficients they are of course very poor. (At one stage of the investigation an attempt was made to redistribute the records in the two sets into a difficult and an easy series. The new correlations thus obtained are those between 'D' and 'E'.)

Table VI. *Ranges of scores: both groups* (*see text*)

	Highest	Mean	Lowest
Revised Kwalwasser	58	31·2	8
	76	*64·7*	*49*
Scores: Series A	9	4·8	2
	8	*6·2*	*4*
Scores: Series B	6	3·7	1
	8	*4·7*	*1*
Liking: Series A	10	6·1	3
	9	*6·3*	*3*
Liking: Series B	10	7·3	3
	8	*5·3*	*2*

Table VII. *Correlations*

	Unselected	All
Kwalwasser and 'page 1'	0·45 ± 0·048	—
Kwalwasser and A	0·28 ± 0·061	0·39 ± 0·052
Kwalwasser and B	—	0·44 ± 0·087
Kw and A *liking*	0·20 ± 0·064	0·02 ± 0·059
Kw and B *liking*	—	−0·30 ± 0·098
'page 1' and A	−0·05 ± 0·064	—
'page 1' and B	0·27 ± 0·127	—
'page 1' and A *liking*	0·31 ± 0·056	—
'page 1' and B *liking*	0·40 ± 0·111	—
A with B	0·43 ± 0·112	0·37 ± 0·090
'D' with 'E'	0·52 ± 0·099	0·32 ± 0·093
A and A *liking*	0·16 ± 0·061	0·15 ± 0·056
B and B *liking*	0·27 ± 0·123	−0·05 ± 0·096
Liking: A and B	0·74 ± 0·062	0·72 ± 0·051

(7) *Conclusions from the correlations obtained*

Other main points which seem to emerge from the correlations are as follows:

(i) *Liking* seems to be an independent and fairly constant factor in this test situation. It was found difficult to devise a technique whereby it could be discovered whether 'understanding' of a composition was correlated with liking for it. Correlating the number of selections liked with the number correct did not really meet the case, and the distributions obtained made it almost impossible to calculate association coefficients for each record separately. Nevertheless, it was found that negative or very low association coefficients were found mainly in cases where either there was a widespread antipathy to music of that type (e.g. A 3), or the music was definitely in an unfamiliar idiom (e.g. A 7, A 9). On the whole it seemed possible to draw the general conclusion that, other things being equal, that music was enjoyed which was correctly understood.

(ii) In some respects 'page 1' was a better or more useful measure of 'musical sophistication' than were the questionnaire scores. This is seen especially in the case of the correlations with *liking*. While there was here no marked connexion between musical knowledge (as measured by the questionnaire) and liking, experience of music of a more direct kind (as measured by 'page 1') correlated with it positively, to a degree statistically significant. On the whole, the indications are that knowledge of music widens the scope of what one can enjoy—a point contrary to the view that the untaught show lack of discrimination, while the sophisticated are unduly eclectic. It is noticeable, however, that in the case of Series B, there is a *negative* correlation with the Kwalwasser scores—as might have been expected in view of the low musical value of some of the B records.

IV. RÉSUMÉ AND DISCUSSION

The following discussion restates the main standpoint of the investigation, and amplifies certain points arising from the experimental procedure and the results.

(1) *Validity of the method of testing*

The object of the experiment was to test musical ability on the basis of the assumption that one way in which the musically gifted individual shows his superiority is in correctly identifying the composer's intention or other authenticated content of a musical composition, whether of a 'programme' character or otherwise, provided that that content is sufficiently clearly expressed. That such an ability exists, the results of the present experiment seem to show. How far it is correlated with other aspects of musical ability cannot be known until results from really satisfactory test material are available. Although many of the selections used were correctly interpreted by only a small proportion of the hearers, it was possible to check the correctness of what may be called the 'official' interpretation as follows:

From the records used, a selection of six was made on which low scores had been obtained; these, together with three others not previously heard, were played to sixteen members of the Unselected group who had listened to both series. Before playing each record, the writer gave some information regarding its title and content, in some cases pointing out certain features which were thought to indicate why the 'official' interpretation was correct and the others wrong. Among other questions, which served partly as 'buffer' questions, the subjects were asked to state whether they thought the suggested interpretation was borne out by the music. This procedure of course lays itself open to the charge of suggestion, but from his personal knowledge of the individuals concerned, the writer does not consider that this factor had much influence on the results obtained. Table VIII shows the numbers of affirmative answers received against the numbers of correct interpretations previously recorded by these subjects. Allowing for a noticeable (and possibly conscious) obstinacy on the part of one or two individuals, and for the few 'reversals' recorded, the results may be taken as fairly reassuring.

Table VIII. *Number of correct responses on first hearing, and number of agreements and non-committal responses on rehearing: sixteen subjects*

Record		First hearing. Correct	Rehearing	
			Agreement	Non-committal
B6	Tortoises	4	9	—
A4	Elephant	6	7	—
B5	Symphony	3	14	1
A6	Kolj Slavenj	1	9	1
A7	Pastorale	5	9	—
B4	Sérénade	2	9	4
	Total	21	57	6

(2) *Possibility of improvement of the test material*

The material might be generally improved in the following way: Each proposed test item would be submitted to as large a body as practicable of really competent critics, and any item on which insufficient unanimity was forthcoming would be discarded. Thus, for example, in the case of the present Series A, a member of the Faculty of Music refused to accept the applicability of the question asked to the Schubert Trio in B♭. On the other hand, a large proportion of the Musical group gave the expected response. How far this was due to actual knowledge of the work would be hard to determine, but it serves to re-emphasize the desirability of unfamiliar material in a test situation such as this. When another large group of subjects becomes available it is hoped to repeat the experiment with practically the same material. If some of the alternatives suggested were altered, possibly only one record (No. 6) would require to be discarded from Series A. Nos. 1 and 2 could be left as shock absorbers, and No. 3 could be made more discriminative by substituting a more tempting alternative for 'solemnity'. Further adjustments or replacements could also be considered.

(3) *Desirability of a further revision of the Kwalwasser test*

In any investigation of musical ability, at any rate in the case of adults and older children, the use of a check of musical knowledge, such as the Kwalwasser test, seems essential. Experience in the present instance indicates that it would be desirable to get rid, as far as possible, of the 'Right minus Wrong' method of scoring, which is used in three sub-tests in both the original Kwalwasser form and the writer's revision. Tests V and VI might be improved by making them a little more difficult and by confining Test V to instruments other than those of the symphony orchestra. The 'page 1' questions might possibly also be made more systematic.

(4) *Applicability to other groups*

In conclusion the writer wishes to stress the fact that the experiment reported is to be regarded as an *approach* to a method of testing, and, further, as an approach from what might be called 'the top'. Ultimately, it might be possible to adapt the method to the testing of quite young children, and the writer hopes, if possible, to work down gradually through the various age groups. Interesting results might also be obtained by testing groups of adults with comparatively little experience of music, though in this radio age such people, whether adults or children, would no doubt be hard to find. Quite apart from the diagnostic value of the test method, the writer has more than once heard the opinion expressed that it could be of considerable use in the teaching of Musical Appreciation.

V. SUMMARY

1. The experiment described was designed to meet the need for a test of musical ability of an untrained type, as distinct from proficiency resulting from education in music.

2. Preliminary experiments suggested that interpretative processes are an important aspect of the experience of, and enjoyment derived from, listening to music. Different individuals were found to possess different degrees of interpretative power; it was therefore thought possible to test this aspect of musical ability quantitatively.

3. Two series, each consisting of ten gramophone records of music of varied types, were drawn up, and report-sheets prepared. The listener was required to state whether he liked each record, and to select the

correct interpretation from a group of four alternatives. The subjects were students of the Faculties of Arts and Music. Despite deficiencies in the material, statistical treatment of the results indicated that ability to interpret music was fairly constant for the individual, and relatively independent of musical training and experience.

4. A check on the legitimacy of postulating a correct interpretation was obtained. It is now hoped to extend the test method to other age groups. Suggestions for the improvement of the test-material are proposed.

I wish to express my indebtedness and thanks to the following:

To the publishers of the Kwalwasser test for permission to publish results based on my revision of that test; to Prof. Sir Donald Tovey and Dr Mary Grierson for permission to test in the Faculty of Music; to all those who volunteered their services as subjects, or gave other assistance.

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THE NATURE OF THE PRACTICAL FACTOR (F)

BY E. J. J. PRICE

(From the Oxford University Institute of Experimental Psychology)

I. *Introduction* (pp. 341–342).
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  (1) *Averages and standard deviations* (p. 346).
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I. INTRODUCTION

Two recent studies have made some inroads into the common-factor structure of intelligence tests of a practical and of a primarily perceptual kind respectively. Alexander (1935) has demonstrated that performance tests involve a factor over and above the Spearman g-factor in these tests; he called it 'F' and proceeded to argue for a concept of 'practical intelligence' in terms of his findings. Koussi (1935) and later Smith (1937), have likewise isolated a factor over and above g, called K, in pencil-and-paper tests which require the perception of geometric and similar two-dimensional shapes, and the eduction of space relations.

Now there are obvious similarities between the two kinds of tests supplying these factors F and K respectively, and one wonders at once whether the two factors are not, after all, one and the same. Performance tests, and paper-and-pencil 'form-relations' tests, both require shapes and their parts to be cognized, and space relations to be educed about them. The same kind of 'mental set' is involved too. One of the best K-factor tests, that called 'Fitting shapes' (Brown & Stephenson (1933)), is merely a set of form-boards represented on paper. The present study seeks to determine, therefore, whether or not K and F factors are one and the same. The problem is as follows: if a matrix is constructed which contains a number of performance tests, a number of pencil-and-paper 'space' or 'form-relations' tests, and a number of reference g-tests, will two factors (other than specifics), Spearman g and one other, be sufficient to account for the correlations? The following evidence suggests that the answer is in the affirmative.

II. METHOD OF EXPERIMENT

(1) *The test variables*

The following tests were used for the matrix required:

Reference g-tests:

No. 1. Stephenson verbal intelligence test. Book I.[1]
No. 2. Stephenson non-verbal intelligence test. Book II.
No. 3. Stephenson non-verbal intelligence test. Book III.

Performance tests:

No. 4. Modified Dearborn-Anderson (see references below).
No. 5. Cylinder construction.
No. 6. Kohs' block design (1920).
No. 7. Woolley blindfold (see references).
No. 8. Alexander's 'Passalong' (1933).

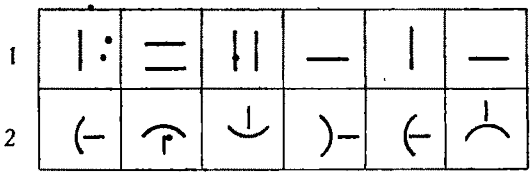
Space-factor pencil-and-paper tests:

No. 9. Fitting shapes.
No. 10. Shape completion.
No. 11. Pool image.

The verbal intelligence test consists of several pages of verbal analogy test units, in four different forms; since it is the only verbal test in the matrix it can be used here as a 'reference' test for Spearman g-factor. The non-verbal intelligence tests are of the kind used by Koussi (1935), and in the Spearman-Holzinger study (1937), and have been shown not to involve K-factor. They will be described elsewhere, and it is sufficient to give a sample only of each at this juncture. Test No. 2 contained seven pages of problems of the kinds shown below:

Pages 1 and 2: non-verbal classification test units.

(1) Select four of the six drawings that are most obviously alike in some way:



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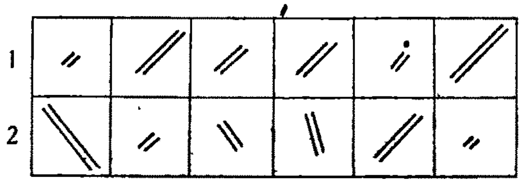
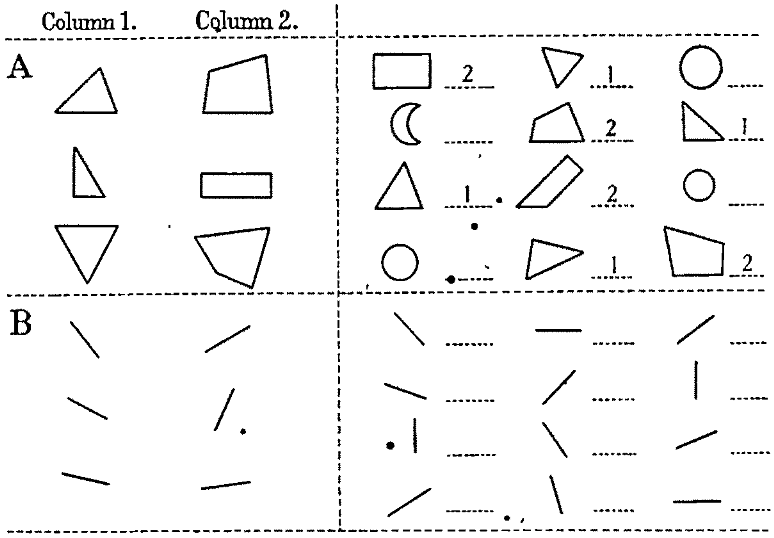
[1] Tests Nos. 1, 2, 3 are standard tests, used in all factor work in the Institute.

Pages 3 and 4: non-verbal series test units.

(2) Two of the six drawings have been interchanged; when put in their correct place the six make a regular series with respect to some feature of the drawings:



Pages 5, 6, and 7. Classification. The three drawings under A are alike in some way; those under B are alike in a different way; find four drawings at the right that are like those under A, and four like those under B:



Test No. 4 contained eighteen items, similar to those used in the Dearborn-Anderson form-board test. Each item consists of a small board (12 × 12 in. approx.) with an inset, into which three pieces of wood can fit; these three pieces serve in turn for all eighteen boards. The subject was given 15 sec. in which to attempt each board, and the number of pieces correctly inserted in that time was taken as the score for that board.

The Cylinder Construction test was devised for this study, with the object of seeing how far *F*-factor extended into three-dimensional constructions. A number of pieces are given which, when put together in a rack ready for the purpose (see Fig. 1), make up a solid wooden cylinder, 6 in. high, 2 in. diameter. Six cylinders have to be constructed in all; the number of pieces correctly assembled in each in one minute is taken as the score for that item.

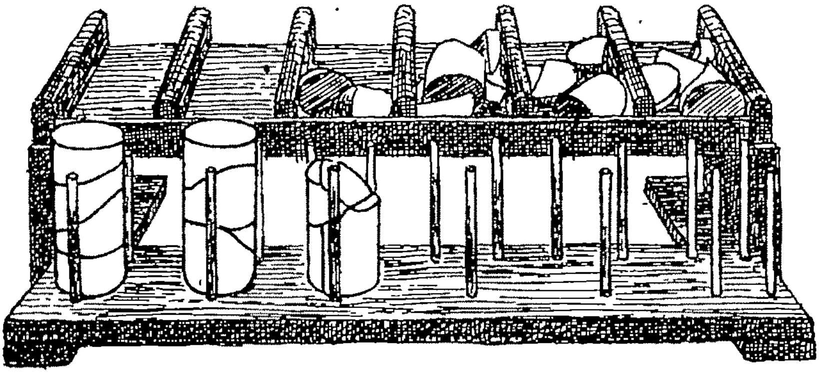


Fig. 1. Cylinder Construction test

Kohs' Block Design Test was the standard test, used by Alexander to demonstrate *F*-factor. Alexander's Passalong test was that described in the *British Journal of Psychology* (1933), except that in making it we decided to make the pieces larger, for ease in manipulation. The Woolley Blindfold test was also made in the laboratory. It consists of twelve wooden shapes fixed on a board (see Fig. 2). Each of these shapes

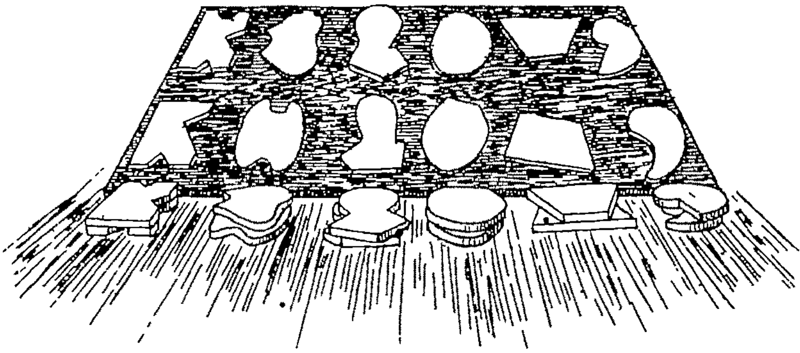


Fig. 2. Woolley test

has an unattached replica. The subject saw neither the fixed pieces nor their counterparts. Both written and verbal explanations of what he was asked to do were given him, and when he fully understood them, he was blindfolded. The test board was then placed before him, and he was allowed to pass his hands over the pieces fixed on the board for 20 sec. At the end of that time he removed his hands from the board, and was given one of the loose pieces to handle for 10 sec.; he was forbidden to touch the board while handling the loose piece. At the end of the 10 sec. the piece was taken away from him. He was then asked to identify its counterpart on the board; for this he was allowed another 10 sec. He was of course blindfolded the whole time, and had to solve the tests by touch, i.e. by haptic sense. Two marks were given for each piece correctly identified. It will be seen that there is a certain resemblance between the upper and lower members of each pair of pieces on the board: if a subject was successful to the extent of finding the right pair, but the wrong member of it, he was awarded one mark.

Samples of the Fitting Shapes test, the Shape Completion tests, and the Pool Image test are given in Figs. 3, 4 and 5.

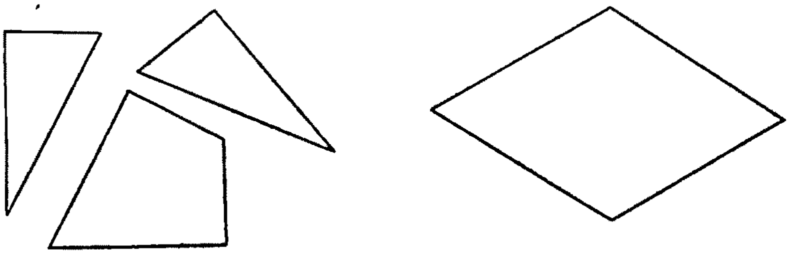


Fig. 3. Fitting Shapes test. Draw lines to show how the right-hand figure should be cut to give the shapes on the left

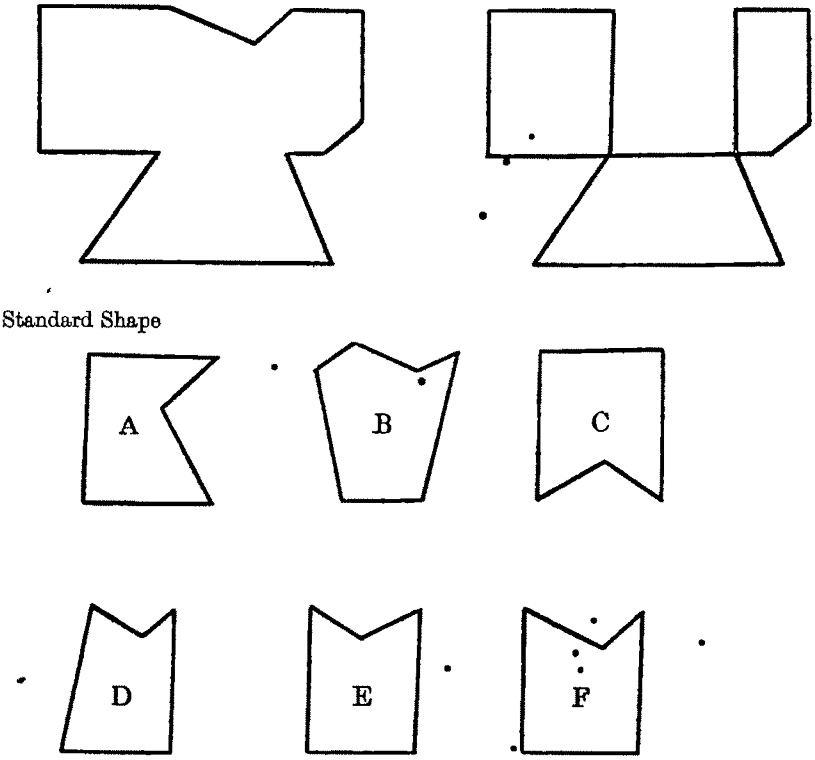


Fig. 4. Shape Completion test. Item 1. Select *one* of these pieces, which, fitted to the right-hand shape above, makes the *standard shape*

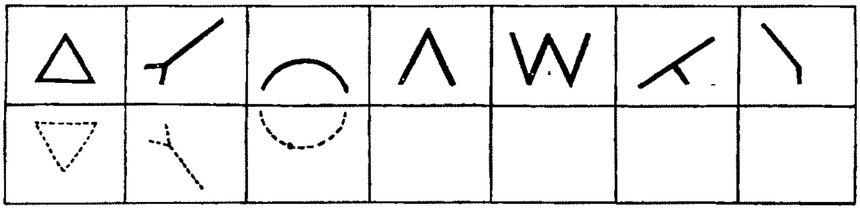


Fig. 5. Pool Image test. Imagine the blank squares to represent pools of still water, and draw in each what you think would be the reflection of the figure above it

(2) *Subjects*

Eighty-five University men students were tested individually by the author. All were between 19 and 22 years of age, were selected at random, and attended either Oxford University (60) or University College, London (25). None had done performance tests of this kind before. A few, of course, had sat for verbal intelligence tests at one time or another. The number of subjects, whilst not large, is sufficient for the present purpose, and represents a considerable amount of individual testing.

III. RESULTS

(1) *Averages and standard deviations*

The mean scores and standard deviations for each of these tests, for the population of eighty-five students, are shown in Table I.

Table I

	Test	Mean	Standard deviation
1.	G–1	61·88	13·50
2.	G–2	66·24	13·35
3.	G–3	13·65	3·52
4.	Dearborn-Anderson	37·38	9·08
5.	Cylinder Construction	16·25	4·88
6.	Kohs' Block Design	18·60	5·62
7.	Woolley	16·08	3·45
8.	Passalong	10·70	4·86
9.	Fitting Shapes	35·16	13·27
10.	Shape Completion	10·60	4·15
11.	Pool Image	15·09	4·03

(2) *Correlations and factor analyses*

Table II gives the product-moment correlations for the eleven variables. They are positive, except for one which is insignificantly negative.

The simplest factor study for our purpose depends upon accepting the *g*-reference values (Tests 1, 2, 3) as the basis of an analysis. This amounts in effect to a Bi-factor analysis, such as Holzinger advocates (1937), which has in fact been the basis of much Spearman analysis. The three g-tests are used as 'reference values', and their own g-saturations are determined in terms of each other;[1] the g-saturations of the other tests are then calculated one at a time in terms of these three as reference values.[2] These two steps give the factor saturations shown in column (i)

Table II. *Intercorrelations of eleven tests* ($N=85$)

	1	2	3	4	5	6	7	8	9	10	11
1		0·493	0·399	0·106	−0·008	0·306	0·284	0·101	0·290	0·149	0·171
2			0·463	0·285	0·328	0·478	0·373	0·386	0·464	0·358	0·384
3				0·251	0·343	0·479	0·169	0·166	0·267	0·308	0·302
4					0·468	0·547	0·133	0·144	0·576	0·535	0·500
5						0·442	0·230	0·191	0·411	0·447	0·427
6							0·341	0·209	0·717	0·594	0·585
7								0·132	0·143	0·267	0·250
8									0·205	0·206	0·105
9										0·486	0·679
10											0·423
11											

1. G–1.
2. G–2.
3. G–3.
4. Dearborn-Anderson.
5. Cylinder Construction.
6. Kohs' Block Design.
7. Woolley.
8. Passalong.
9. Fitting Shapes.
10. Shape Completion.
11. Pool Image.

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[1] The g-saturation of test 1 in terms of 2 and 3 is given by:

$$r_{1g}=\left\{\frac{r_{12}\cdot r_{13}}{r_{23}}\right\}^{\frac{1}{2}}.$$

Similarly for tests 2 and 3.

[2] The g-saturation of any of the tests 4–11 is calculated by adding them one at a time to the matrix for variables 1, 2, and 3. This is most conveniently done by using an adaptation of the Spearman formula (21) viz.

$$r_{ag}=\left\{\frac{A^2-A'}{T}\right\}^{\frac{1}{2}},$$

where A is the sum of the correlations between test 'a' and the reference values 1, 2, 3; (i.e. correlations $r_{a1}+r_{a2}+r_{a3}+\ldots$) and A' is the sum of the squares of these correlations and T the sum of the intercorrelations between the reference values (i.e. $r_{12}+r_{13}+r_{14}\ldots$).

of Table III. Using these saturations, *residual* correlations are next calculated for Table II, with the results shown in Table IV. In Table IV the tests 1, 2, and 3 are omitted; all the residuals involving these tests are insignificant, their mean is zero (−0·00639), and each residual is itself insignificant as judged by the standard error of the difference between the original correlation and the theoretical one deriving from the g-factor content. The residuals for the remaining tests, however, are all positive (except $r_{8,11}$ and $r_{9,7}$). Assuming that one common factor (x) is sufficient to account for these residuals, we calculate the x-factor saturation of each test in turn in terms of all the other tests in the matrix, using Spearman formula (21) (1927). This gives the x-factor saturations shown in Table III, column (ii).

Table III

Test	Factor I (g)	Factor II (x)
1. G–1	0·652	—
2. G–2	0·756	—
3. G–3	0·612	—
4. Dearborn-Anderson	0·314	0·698
5. Cylinder Construction	0·340	0·476
6. Kohs's Block Design	0·618	0·559
7. Woolley	0·402	0·050
8. Passalong	0·297	0·062
9. Fitting Shapes	0·620	0·548
10. Shape Completion	0·393	0·549
11. Pool Image	0·415	0·589

Table IV. *Residual correlations of tests* 4–11 *after removal of g-factor*

	4	5	6	7	8	9	10	11
4		0·36109	0·35297	0·00688	0·05045	0·38185	0·41226	0·36960
5			0·23206	0·09373	0·09014	0·20006	0·31358	0·28567
6				0·07404	0·02543	0·33414	0·35142	0·32872
7					0·01302	−0·10648	0·10888	0·08267
8						0·02050	0·08911	−0·01791
9							0·24163	0·42206
10								0·26015
11								

Inspection shows that the residuals after the removal of the x-factor are insignificant. The biggest of these second residuals is 0·09929; their mean is 0·00454. None is significant as judged by the standard error of the difference between the original correlation (first-factor residual) and the theoretical correlation deriving from the x-factor content.

(3) *Results of a multiple-factor analysis*

In addition to the above Spearman analysis a further examination of the data was made, using the Thurstone centroid method. In Table V are shown the factor loadings obtained from this analysis, while Table VI (see p. 350) contains the residual correlations after removal of the two factors. These residuals are insignificant. The general resemblance between the loadings obtained by the second analysis and those of the Spearman analysis is very clear; rotation of axes would make it still more evident.

Table V. *Factor loadings for the eleven tests, using the Thurstone centroid method*

Test	Factor I	Factor II
1. G–1	0·472	−0·461
2. G–2	0·687	−0·418
3. G–3	0·553	−0·219
4. Dearborn-Anderson	0·629	0·389
5. Cylinder Construction	0·574	0·221
6. Kohs' Block Design	0·826	0·140
7. Woolley	0·411	−0·210
8. Passalong	0·340	−0·188
9. Fitting Shapes	0·756	0·309
10. Shape Completion	0·666	0·207
11. Pool Image	0·687	0·280

IV. CONCLUSION

Thus the two analyses, the Spearman and the Thurstone, both point to the same conclusion, viz. that the second factor in performance tests is the same as that in K-factor tests. K-factor of Koussi's study (1935) would seem to be the same as F-factor in Alexander's, as far as our data are concerned. It would seem best to retain the designation K to stand for the factor in either performance tests or in pencil-and-paper 'form-relations' tests, since what is apparently critical in both kinds of tests is the cognition and operation of 'form relations'. It is recognized, of course, that something intrinsic to performance might be critical for children; for our students, however, there would seem to be nothing critical in the performance as such.

The analysis makes it clear that there is not a group factor running through *all* performance tests, for the second factor saturations of the

Woolley and Passalong tests are negligible or non-existent. K-factor is not involved in the Passalong test, and it is not difficult to see why this should be so. The test is a performance test, it is true; but form and shape and spatial relations enter into it to a very slight extent. The test calls for that kind of thinking which is covered by g-factor. In theory, the subject should be able to solve the later, more difficult items by reasoning upon his experience of the earlier ones. Thus the last two items present no difficulty to one who realizes that as they contain the same blocks as the last but one, differently arranged, the problem is merely to work back to the simpler arrangement. Actually this is not realized by the great majority of subjects; not six out of the eighty-five subjects tested declared that they could see any logical connexion between the various items. Nearly all declared that they had approached each one as a new problem, and, indeed, it was apparent from the random and fumbling approach of the great majority to each of the harder items, that they set about

Table VI. *Residual correlations after removal of two factors*
(Thurstone centroid method)

1	2	3	4	5	6	7	8	9	10	11
1	0·008	0·062	−0·016	0·135	−0·027	0·012	−0·131	−0·109	0·040	−0·007
2		−0·009	−0·016	−0·028	0·030	0·003	0·073	−0·074	0·013	−0·029
3			0·012	−0·074	−0·053	−0·104	−0·064	0·083	0·015	0·017
4				0·021	−0·027	0·044	−0·004	−0·020	−0·035	−0·041
5					−0·063	−0·040	−0·038	−0·091	0·019	−0·029
6						−0·031	0·045	0·050	0·015	−0·021
7							−0·048	0·103	−0·036	−0·027
8								−0·007	−0·019	0·076
9									−0·081	0·073
10										−0·093
11										

them 'without an idea in their heads' of a logical kind. The later items in the Passalong test can be solved only by luck, or by explicit formulation of the 'trick' involved in the test. The Woolley test also failed to show correlation with a factor other than the general factor; the results obtained here throw no light on the relationship, if any, between haptic perception and K-factor.

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AN INVESTIGATION OF ABILITY IN WORK IN GROUPS AND IN ISOLATION

By N. P. MUKERJI

I. *Statement of the problem* (p. 352).
II. *Procedure* (pp. 352–353).
III. *Analysis of results* (pp. 353–354).
IV. *Oscillation* (pp. 354–356).
V. *Summary* (p. 356).
References (p. 356).

I. STATEMENT OF THE PROBLEM

AN effort has been made in the present article to examine if there be any difference in ability and in oscillation when a particular type of work is carried out by the same subjects working either in a group or in isolation.

II. PROCEDURE

Thirty-one children, eighteen boys and thirteen girls, from an English elementary school[1] participated in the experiments; their ages ranged from 11 yr. 3 mo. to 13 yr. 8 mo. Two tests, viz. Cancellation and Naming Capitals, were first given to the subjects in mixed groups of not less than ten children. The tests were then repeated, but this time the subjects worked in isolation. In the Cancellation test they had to cancel out Ts and Xs only. In the Naming Capitals test, letters were typed in lines running in zigzags all over a sheet, and the subjects had to write S under the letters which consisted of straight lines only, e.g. A, K, L, M, and C under those which consisted wholly or in part of curved lines, e.g. O, P, Q, B.

The subjects were instructed to draw a line across the item they had reached whenever they heard a signal given at the end of each minute of work. Again, every 10 sec. after receiving a different signal they had to draw a line under the last item of work. Thus the whole test, which lasted for 5 min., was divided into thirty equal divisions of work.

Care was taken to choose only those subjects for the present experiments who had participated in the writer's previous experiments in which the tests mentioned above were included; this precaution was taken in order to minimize the effect of practice on the required data. Further, the subjects were given a half minute of practice trial before beginning work.

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[1] The author wishes to express his thanks to Prof. J. C. Flugel for his encouragement in this investigation and to the staff of the Isle of Dogs School, London, especially to Mr F. J. Shatwell, for their co-operation which made the study possible.

Each right answer scored one mark; the total number of marks obtained in 5 min. of work constituted the ability score. For the determination of reliability, the split-half method was used.

Henceforth, for the sake of abbreviation, the tests of Cancellation and Naming Capitals will be referred as T–X test and C–S test respectively.

III. ANALYSIS OF RESULTS

The reliability of the tests is shown in Table I. The correlations between the results of the two tests obtained in groups and in isolation is shown in Table II; and the correlations between the two tests under these different conditions in Table III. Table IV shows the average ability in the different tests. In the T–X test twenty-six subjects, i.e. 83·8 % of the cases, showed an increase in ability when they worked in groups. The average gain in the score (among the twenty-six subjects) was 27·7 %. In the case of the C–S test increased ability in group work was manifested by twenty-seven subjects, i.e. 87 %, and the average gain here was 20·0 %.

Table I

	Test	
Groups	T–X	0·82 (0·039)
	C–S	0·77 (0·049)
Isolation	T–X	0·82 (0·039)
	C–S	0·87 (0·029)

(Figures within brackets denote P.E.)

Table II

Test	
T–X	0·53 (0·091)
C–S	0·73 (0·059)

Table III

Groups	T–X and S–C tests	0·61 (0·076)
Isolation	Do.	0·42 (0·099)

Table IV

	Tests	Average ability	S.D.
Groups	T–X	149 (4·1)	34·2
	C–S	160 (5·3)	43·2
Isolation	T–X	114 (2·7)	23·0
	C–S	132 (2·8)	23·9

With regard to sex differences in ability the data collected here may not be highly significant as the number of subjects is comparatively small, but we may note that the difference is marked. It is shown in Table V

Table V

Pooled tests	Boys	Girls
In groups	153	156
In isolation	121	127

that the average ability among the boys is practically the same as for the girls, but there seems to be a probability of the girls scoring higher than the boys if N were larger. Taking the total raw scores in ability into consideration it is found on pooling the tests that the boys gained in group work by 61·5 % whereas the girls gained only 38·5 %.

IV. OSCILLATION

There are two chief possible ways of determining variability in oscillation. The first is by calculating the difference between the mean of the total work and the amount done in each short period of 10 sec. This procedure in itself is quite justifiable; but it can be argued that if the work gradually improves as the result of practice or of similar effects, then in adopting this method we shall be taking this improvement into account as well as oscillation. Since our concern is with oscillation and not with improvement, it is safer here to follow the other method which consists in determining the difference between successive individual measurements. This method has one advantage over the former: it measures short-period oscillation with a minimum of disturbance by any continuous improvement or deterioration of the work. It may be gathered from Table VI that oscillation as a function of ability remains practically unchanged whether tested in groups or in isolation. Comparing the total raw scores we find that the oscillation in group work is 28 % more than that in isolation. Thus, it may be stated at this stage that oscillation as a function of ability is positively related to it.

Table VI

	Tests	Average oscillation	S.D.
Groups	T–X	55·8 (1·4)	11·9
	C–S	56·3 (2·1)	17·8
Isolation	T–X	31·7 (0·6)	5·0
	C–S	31·7 (0·9)	8·2

Table VII shows that oscillation as such does not vary extensively in the different sexes. The correlation between ability and absolute oscillation is shown in Table VIII. The correlations, though not uniformly high, are nevertheless all positive. Interpreting the above results, it may be said that the greater the ability, the greater the oscillation; but this effect is more marked in group work than in the work in isolation.

Another method of treating the oscillation data was tried by determining relative oscillation; the oscillation measurements already obtained (absolute oscillation) were calculated as percentages of the subject's total output. The results are shown in Table IX. We get here a result that contrasts strongly with the previous one; oscillation shows a negative relation to ability, and on the whole this relation is close. In general, the more able subjects show less oscillation, and this fact is corroborated by several previous workers(1,2,3).

Table VII

Average oscillation among	In groups	In isolation
Boys	54	32
Girls	58	31

Table VIII

	Test	
Groups	T–X	0·51 (0·089)
	C–S	0·54 (0·085)
Isolation	T–X	0·21 (0·115)
	C–S	0·17 (0·117)

Table IX

	Tests	
Groups	T–X	−0·54 (0·085)
	C–S	−0·28 (0·109)
Isolation	T–X	−0·60 (0·077)
	C–S	−0·46 (0·095)

Table X

	Tests	
Groups	T–X	37·4
	C–S	36·5
Isolation	T–X	28·2
	C–S	24·3

Table XI

Average relative oscillation among	In groups	In isolation
Boys	36·5	27·9
Girls	37·5	25·2

The average relative oscillation in groups and in isolation is shown in Table X. Comparing these results with those in Table IV, we reach conclusions which are in accordance with the correlations given in Table IX, namely, that average ability in the different tests shows an inverse relationship to average relative oscillation. Nevertheless, relative oscillation is greater in groups than in isolation. The results of the pooled tests, shown in Table XI, indicate that girls oscillate more when working in groups whereas boys oscillate more in isolation.

V. SUMMARY

1. Thirty-one trained subjects (eighteen boys and thirteen girls) were given Cancellation and Naming Capitals tests in mixed groups of about ten. The tests were repeated on the same subjects, but this time they worked in isolation.

2. Ability in group work exceeded that in isolation by 20·0 % to 27·7 %. This advantage of group work is greater for boys than for girls, though the average score in ability is higher among the girls than the boys.

3. Absolute oscillation, between successive individual measurements, is positively related to ability. The oscillation in group work is greater than that in isolation, but the amount of oscillation is practically equal in boys and in girls.

4. Relative oscillation, determined in terms of the percentage of subject's total output, is negatively related to ability, i.e. the better subjects oscillate relatively less; however, relative oscillation is greater in group work than in isolation. Further, it is noticeable that relative oscillation is greater in the case of girls when working in groups whereas it tends to be less in boys under similar circumstances.

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WEIGHTING FOR BATTERY RELIABILITY AND PREDICTION

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I. *The Spearman-Brown formula* (pp. 357–358).
II. *Battery reliability* (pp. 358–359).
III. *Weighting for maximum battery reliability* (pp. 359–360).
IV. *A numerical example* (pp. 361–364).
V. *Reliability* versus *prediction of a criterion* (pp. 364–365).
VI. *Summary* (p. 365).
References (p. 366).

I. THE SPEARMAN-BROWN FORMULA

IF we have a split test and find the correlation r of one half with the other we can, as is well known, estimate the correlation which the whole test will have with an alternative form of the whole by the formula

$$\frac{2r}{1+r},$$

which is therefore an estimate of the reliability of the whole test (1, 2). The other whole test with which that which we possess is supposed to be correlated is of course imaginary. It is assumed that its halves would also correlate r with one another, and also that each of them would correlate r with each of the actual halves of the existing test. Also each of the four halves is assumed to have the same standard deviation. These assumptions can be clearly shown in a 'pooling square' (as I call it) by which Spearman's formula for the correlations of sums and differences can be readily applied. We have four half-tests h_1, h_2, h_1', h_2' (of which the last two are imaginary) and their intercorrelations:

	h_1	h_2	h_1'	h_2'
h_1	1	r	r	r
h_2	r	1	r	r
h_1'	r	r	1	r
h_2'	r	r	r	1

in which table only the r between h_1 and h_2 is an observed quantity, all the other r's being assumed. In such a pooling square (the standard deviations being equal) the correlation between the battery h_1+h_2 and the battery $h_1'+h_2'$ is

$$\frac{\text{sum in N.E. quadrant}}{\sqrt{[(\text{sum in N.W.})\ (\text{sum in S.E.})]}}$$

$$=\frac{4r}{\sqrt{[(2+2r)\ (2+2r)]}}=\frac{2r}{1+r},$$

the Spearman-Brown formula. My object in writing this preliminary section is to prepare the way for the idea of the correlation of a *weighted* battery of tests with an imaginary similar and similarly weighted battery. For the Spearman-Brown formula envisages the simplest case of this idea, the (half-)tests in the battery being here alternative forms of the same (half-) tests, and the weights being equal.

II. BATTERY RELIABILITY

If in the above pooling square h_1 and h_2 were not halves of a test, but *different tests* in a battery of two tests, we would have the first approach to the more general case to be discussed later. We can then imagine h_1' to be an alternative form of h_1, and h_2' to be an alternative form of h_2. If we represent the correlation between h_1 and h_2 by r_{12}, we can, with the same justification as in § I, assume the correlation between h_1' and h_2' to be also r_{12}. Moreover the correlation between h_1 and h_2', and that between h_2 and h_1', can also be taken as r_{12}.

The correlation between h_1 and h_1', however, is now a different matter. It is the reliability of h_1, which we shall call r_{11}, and which we may assume to be estimated by splitting h_1 and 'stepping up' the split reliability by the Spearman-Brown formula. Similarly the correlation between h_2 and h_2' is r_{22}, the reliability of h_2. The pooling square is now

	h_1	h_2	h_1'	h_2'
h_1	1	r_{12}	r_{11}	r_{12}
h_2	r_{12}	1	r_{12}	r_{22}
h_1'	r_{11}	r_{12}	1	r_{12}
h_2'	r_{12}	r_{22}	r_{12}	1

and by the same principle as before the correlation of the battery h_1+h_2 with the (imaginary) battery $h_1'+h_2'$ is found (by summing the quadrants) to be

$$\frac{r_{11}+r_{22}+2r_{12}}{2+2r_{12}},$$

provided the standard deviations have all been equalized. This is the estimated reliability of the battery formed by the straight sum of its constituent tests, calculated from the intercorrelations and separate reliabilities of those constituent tests.

If the reliability of the battery formed by a *weighted* sum of the constituent tests is required, the pooling square (which is of course only a way of envisaging Spearman's formula) gives the result almost equally easily. Each column *and each row* of the pooling square has to be multiplied by the appropriate weight before the quadrants are summed. Taking the weights to be w_1 and w_2 we have therefore, on thus multiplying the rows and columns, the square

	h_1	h_2	h_1'	h_2'
h_1	w_1^2	$w_1 w_2 r_{12}$	$w_1^2 r_{11}$	$w_1 w_2 r_{12}$
h_2	$w_1 w_2 r_{12}$	w_2^2	$w_1 w_2 r_{12}$	$w_2^2 r_{22}$
h_1'	$w_1^2 r_{11}$	$w_1 w_2 r_{12}$	w_1^2	$w_1 w_2 r_{12}$
h_2'	$w_1 w_2 r_{12}$	$w_2^2 r_{22}$	$w_1 w_2 r_{12}$	w_2^2

and the estimated reliability of the weighted battery is

$$\frac{w_1^2 r_{11} + w_2^2 r_{22} + 2 w_1 w_2 r_{12}}{w_1^2 + w_2^2 + 2 w_1 w_2 r_{12}}.$$

If the scores of the constituent tests have not been standardized, a σ with the appropriate subscript must accompany each w in this formula, for standard deviations act exactly like weights.

III. WEIGHTING FOR MAXIMUM BATTERY RELIABILITY

It is at once evident that in a given battery the estimated reliability of the weighted battery will depend not only on the test reliabilities and intercorrelations but on the weights chosen, and that there will exist a set of weights which make the estimated reliability a maximum. In the above simple case of two tests these 'weights for maximum battery reliability' can be found by elementary mathematical methods. But I shall consider the case of a larger battery and ask how then to find these weights. This is a special case of a more general problem solved by Hotelling(3). I shall first give the mathematical solution, and then a numerical example which will enable the non-mathematical reader to carry out the necessary calculations. To save space in printing I shall suppose the battery to contain three tests; but the method and formulae apply to any number.

Let the matrix of intercorrelations (including the test reliabilities) be as follows:

$$\begin{array}{c|ccc|ccc|}
 & z_1 & z_2 & z_3 & z_1' & z_2' & z_3' \\
\hline
z_1 & 1 & r_{12} & r_{13} & r_{11} & r_{12} & r_{13} \\
z_2 & r_{12} & 1 & r_{23} & r_{12} & r_{22} & r_{23} \\
z_3 & r_{13} & r_{23} & 1 & r_{13} & r_{23} & r_{33} \\
\hline
z_1' & r_{11} & r_{12} & r_{13} & 1 & r_{12} & r_{13} \\
z_2' & r_{12} & r_{22} & r_{23} & r_{12} & 1 & r_{23} \\
z_3' & r_{13} & r_{23} & r_{33} & r_{13} & r_{23} & 1 \\
\hline
\end{array}$$

$$= \begin{array}{c|c} A & C \\ \hline C & A \end{array}.$$

Here the correlations in the N.W. quadrant are experimental observations, and the reliabilities r_{11}, r_{22} and r_{33} are also supposed known, probably from splitting the tests z_1, z_2 and z_3 and 'stepping up' by the Spearman-Brown formula. The correlations r_{ij} in the N.E., S.W., and S.E. quadrants are assumptions, made with the same justification as the similar assumptions made in deducing the Spearman-Brown formula.

The capital letters A and C in the abbreviated version of the large matrix stand for the quadrant matrices. Thus

$$C = \begin{bmatrix} r_{11} & r_{12} & r_{13} \\ r_{12} & r_{22} & r_{23} \\ r_{13} & r_{23} & r_{33} \end{bmatrix}.$$

Then the method of finding the weights which give maximum reliability is to solve the equation

$$|CA^{-1}C - \lambda A| = 0$$

for its largest root λ_1; the weights in question are then any row of the adjugate of the matrix

$$(CA^{-1}C - \lambda_1 A),$$

and the maximum attainable reliability is the square root of λ_1.

These weights are to be applied to the constituent tests with equalized standard deviations. If the standard deviations are unequal, the weights must be divided, each by the corresponding standard deviation.

IV. A NUMERICAL EXAMPLE

Let the intercorrelations be as follows:

	z_1	z_2	z_3
z_1	1	0·482	0·617
z_2	0·482	1	0·397
z_3	0·617	0·397	1

and the estimated reliabilities 0·86, 0·73 and 0·83. (These data are taken from Brigham's *Study of Error*, 1932, p. 352.) The first step in the calculation is to find the triple matrix product $CA^{-1}C$. By far the best method of doing this is Aitken's (4), its advantages being more apparent when the number of tests is larger than in our example. A preliminary simplification can be carried out, since the matrices A and C differ only in their diagonal. If we write $D=A-C$, the diagonal matrix formed of the defects from unity of the reliabilities, i.e. in our case

$$\begin{bmatrix} 0{\cdot}14 & \cdot & \cdot \\ \cdot & 0{\cdot}27 & \cdot \\ \cdot & \cdot & 0{\cdot}17 \end{bmatrix}$$

it can easily be shown that

$$CA^{-1}C=A-2D+DA^{-1}D,$$

so that we have the easier task of calculating $DA^{-1}D$. This is done as follows. Write the three matrices in the form

$$\begin{array}{c|c} A & D \\ \hline -D & \cdot \end{array}$$

and condense by pivotal condensation till all the numbers are transferred into the south-east quadrant, which will then be the product-matrix $DA^{-1}D$. The process of pivotal condensation will be clear from the numerical example worked out in Table I. The top left-hand number (unity) is taken as 'pivot' and all the tetrad-differences of which it is a corner are set down in order. Thus 0·7677 is the value of $1-0{\cdot}482^2$, and 0·0996 is $0{\cdot}397-0{\cdot}482\times 0{\cdot}617$. The whole matrix of six rows and six columns is thus reduced to one of five rows and five columns.

The same process is repeated, 0·7677 being taken as pivot. But before calculating the tetrad differences, since they all have to be divided by the pivot, it is convenient to divide the whole top row by 0·7677. Thus the row

$$\begin{array}{lcc|ccc|c} & 0{\cdot}7677 & 0{\cdot}0996 & -0{\cdot}0675 & 0{\cdot}2700 & \cdot & 1{\cdot}0698 \\ \text{becomes} & 1{\cdot}0000 & 0{\cdot}1297 & -0{\cdot}0879 & 0{\cdot}3517 & \cdot & 1{\cdot}3935. \end{array}$$

This is written on the bottom edge of a slip of paper and fastened by two paper clips so as to replace the original row beginning with 0·7677. The pivot is now again unity, and the next slab of the calculation is formed by the tetrad-differences which have this pivot as one corner: thus 0·6064 is 0·6193 − 0·0996 × 0·1297 and so on with the others.

Table I

1·000	0·482	0·617	0·140	.	.	2·239
0·482	1·000	0·397	.	0·270	.	2·149
0·617	0·397	1·000	.	.	0·170	2·184
−0·140	.	.	.	.	.	−0·140
.	−0·270	.	.	.	.	−0·270
.	.	−0·170	.	.	.	−0·170
	0·7677	0·0996	−0·0675	0·2700	.	1·0698
	0·0996	0·6193	−0·0864	.	0·1700	0·8025
	0 0657	0·0864	0·0196	.	.	0·1735
	−0·2700	.	.	.	.	−0·2700
	.	−0·1700	.	.	.	−0·1700
		0·6064	−0·0776	−0·0350	0·1700	0·6638
		0·0776	0·0255	−0·0237	.	0·0794
		0·0350	−0·0237	0·0950	.	0·1062
		−0·1700	.	.	.	−0·1700
			0·0354	−0·0192	−0·0218	−0·0056
			−0·0192	0·0970	−0·0098	0·0680
			−0·0218	−0·0098	0·0477	0·0161

Exactly the same process is repeated as often as necessary, until there are no numbers left in the left-hand block. The matrix $DA^{-1}D$ then appears in the middle block. The fact that it is symmetrical can either be used as a check, or to save about half the calculations all the way down, in which case, however, the signs have to be cared for. The check column is the sum of the row, and ought to agree in each row with the tetrad-difference formed from the appropriate rows in the preceding slab. If decimals are discarded the check column will show discrepancies in the last decimal kept, which should not exceed one or two places.

To form $CA^{-1}C$ we now have

$$
\begin{aligned}
CA^{-1}C &= A-2D+DA^{-1}D \\
&= \begin{bmatrix} 1 & 0{\cdot}482 & 0{\cdot}617 \\ 0{\cdot}482 & 1 & 0{\cdot}397 \\ 0{\cdot}617 & 0{\cdot}397 & 1 \end{bmatrix} - \begin{bmatrix} 0{\cdot}280 & . & . \\ . & 0{\cdot}540 & . \\ . & . & 0{\cdot}340 \end{bmatrix} \\
&\quad + \begin{bmatrix} 0{\cdot}035 & -0{\cdot}019 & -0{\cdot}022 \\ -0{\cdot}019 & 0{\cdot}097 & -0{\cdot}010 \\ -0{\cdot}022 & -0{\cdot}010 & 0{\cdot}048 \end{bmatrix} = \begin{bmatrix} 0{\cdot}755 & 0{\cdot}463 & 0{\cdot}595 \\ 0{\cdot}463 & 0{\cdot}557 & 0{\cdot}387 \\ 0{\cdot}595 & 0{\cdot}387 & 0{\cdot}708 \end{bmatrix}.
\end{aligned}
$$

The equation for finding λ_1, the square of the maximum attainable reliability, is then

$$|CA^{-1}C-\lambda A|=0,$$

that is,

$$\begin{vmatrix} 0{\cdot}755-\lambda & 0{\cdot}463-0{\cdot}482\lambda & 0{\cdot}595-0{\cdot}617\lambda \\ 0{\cdot}463-0{\cdot}482\lambda & 0{\cdot}557-\lambda & 0{\cdot}387-0{\cdot}397\lambda \\ 0{\cdot}595-0{\cdot}617\lambda & 0{\cdot}387-0{\cdot}397\lambda & 0{\cdot}708-\lambda \end{vmatrix}=0.$$

This is a cubic, and the root required is the largest root. It is a great drawback that at this point an equation of the *n*th degree has to be solved, but luckily we always know approximately where the largest root lies, for it must be between the square of the largest single-test reliability, and unity: that is, in our case, between $0{\cdot}86^2$ and 1·00. In my experience, therefore, the easiest procedure at this stage is to evaluate the above determinant (by the same process[1] of pivotal condensation) for trial values of λ beginning with one a little larger than $0{\cdot}86^2=0{\cdot}7396$ and endeavouring to bracket the value which will make the determinant vanish. Thus if we try $\lambda=0{\cdot}8$ we have to evaluate the determinant

$$\begin{vmatrix} -0{\cdot}045 & 0{\cdot}077 & 0{\cdot}101 \\ 0{\cdot}077 & -0{\cdot}243 & 0{\cdot}069 \\ 0{\cdot}101 & 0{\cdot}069 & -0{\cdot}092 \end{vmatrix}=+0{\cdot}0033.$$

For $\lambda=0{\cdot}755$ the determinant $=+0{\cdot}0078$,
,, $\lambda=0{\cdot}9$,, $=-0{\cdot}0086$,
,, $\lambda=0{\cdot}83$,, $=+0{\cdot}0007$,
,, $\lambda=0{\cdot}836$,, $=+0{\cdot}0001$.

We take $\lambda=0{\cdot}836$ as approximately the largest root, and

$$\sqrt{0{\cdot}836}=0{\cdot}914$$

as the maximum reliability attainable. The weights giving this will be proportional to any row of the adjugate of $(CA^{-1}C-0{\cdot}836A)$, i.e. of

$$\begin{bmatrix} -0{\cdot}081 & 0{\cdot}060 & 0{\cdot}079 \\ 0{\cdot}060 & -0{\cdot}279 & 0{\cdot}055 \\ 0{\cdot}079 & 0{\cdot}055 & -0{\cdot}128 \end{bmatrix},$$

that is to the minors got by crossing out any one row, and then each column in turn. We find the weights therefore to be approximately in the ratio

$$1 \quad 0{\cdot}36 \quad 0{\cdot}76,$$

---

[1] The determinant is condensed in the manner already shown (each pivot being converted into unity) until it is reduced to one number. The value of the determinant is then the product of all the *unconverted* pivots, including the last number arrived at. See Aitken (4).

We can, by a pooling square, check this result, by testing what reliability these weights would give, on the assumptions we are making. We set out the whole 6 × 6 matrix (though because of its symmetry half would serve), multiply rows and columns by these weights, and sum the quadrants:

	1	0·36	0·76	1	0·36	0·76
1	1·000	0·482	0·617	0·860	0·482	0·617
0·36	0·482	1·000	0·397	0·482	0·730	0·397
0·76	0·617	0·397	1·000	0·617	0·397	0·830
1	0·860	0·482	0·617	1·000	0·482	0·617
0·36	0·482	0·730	0·397	0·482	1·000	0·397
0·76	0·617	0·397	0·830	0·617	0·397	1·000

We obtain

3·209	2·936
2·936	3·209

and a reliability of

$$\frac{2{\cdot}936}{3{\cdot}209}=0{\cdot}915$$

in agreement with our expectation ($\sqrt{0{\cdot}836}=0{\cdot}914$).

V. RELIABILITY *VERSUS* PREDICTION OF A CRITERION

These weights for maximum battery reliability are of course different from the regression weights for the best prediction of an external criterion. Indeed the best reliability weights may give poor prediction, and the best prediction weights may give poor battery reliability. To illustrate this I choose a small and extreme example, admittedly not likely to occur in practice, but showing the principle involved with clearness. Let two tests z_1 and z_2, and an external criterion z_0, have the intercorrelations

	z_0	z_1	z_2
z_0	1·0	0·3	0·4
z_1	0·3	1·0	−0·4
z_2	0·4	−0·4	1·0

Also let the reliabilities of z_1 and z_2 be 0·5 and 0·8 respectively. With these data we can calculate the regression weights to give the best prediction of z_0 from a weighted battery of z_1 and z_2; and by a 4 × 4 pooling square we can find the reliability this weighted battery is likely to have.

On the other hand we can, by the method of §§ III and IV, find the best weights to give maximum reliability to a battery composed of z_1 and z_2;

and by a pooling square covering z_0, z_1 and z_2 we can find what correlation (i.e. prediction) these weights will give between the battery and z_0.

If, for convenience of comparison, we call the weight of z_2 unity in each case we find the following results:

Weights			
z_1	z_2	Prediction	Battery reliability
0·885	1·000	0·642 (max.)	0·450
−0·231	1·000	0·297	0·817 (max.)

We thus find that (in this example) the best prediction weights give an estimated battery reliability which is less than the multiple correlation, and less than the reliability of either test taken alone. On the other hand, the best weights for battery reliability give a prediction which is less than the correlation of either single test with the criterion. In this example weighting for prediction spoils reliability, and weighting for reliability spoils prediction. It seems natural to search for weights which will make battery reliability equal to prediction, and both as large as possible under this condition. In this simple case such weights can be found (though the process involves solving a quartic). They are weights of 0·5284 to 1·0000, and they give prediction equal to reliability, and both equal to 0·604. I hope that a young colleague to whom I am suggesting this problem may find a practicable way of calculating, for a battery larger than two, the weights which thus make the best of both prediction and battery reliability. I think also that the calculations here given for finding weights for maximum battery reliability will probably prove susceptible of simplification.

VI. SUMMARY

The idea is mooted of an estimated battery reliability based on the reliabilities of the single tests and their weights in the battery. A method is given, with a numerical example, by which to calculate the weights which give the maximum battery reliability. This method is a special case of Hotelling's 'Most predictable criterion'. It is pointed out that prediction weights and reliability weights may conflict, and in a small example those weights are calculated which make prediction and reliability equal at the highest possible value under this condition.

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TWELFTH INTERNATIONAL CONGRESS OF PSYCHOLOGY

Owing to the present war conditions, the Twelfth International Congress of Psychology, which was planned to take place in Edinburgh, Scotland, in July 1940, has been postponed, though not abandoned. The invitation of the City and University of Edinburgh is not withdrawn, and the Local Organization Committee hopes that the Twelfth Congress may still meet in Edinburgh when circumstances permit the continuation and completion of the preparation.

JAMES DREVER, *President of the Committee of Organization.*
GODFREY H. THOMSON, *General Secretary.*

PUBLICATIONS RECENTLY RECEIVED

The Study of Society: Methods and Problems. Edited by F. C. BARTLETT, M. GINSBERG, E. J. LINDGREN, and R. H. THOULESS. London: Kegan Paul. 1939. Pp. xii+498. 10*s.* 6*d.*

This important book is the result, as the Preface makes clear, of an interesting and successful adventure in co-operation. A group of nearly two dozen scholars during a period of several years has patiently worked over the material embodied in it, and consequently, while each chapter remains the responsibility of the one who wrote it, yet each has the authority of the whole group behind it. The nature of the book makes this assurance of detailed and highly-informed criticism before publication of high importance. There have been other cases in which students have taken part in systematic and continued discussions from which a useful volume of collected essays has resulted, but it is a special merit of the collaborators in the production of the volume under review that they in no sense form a 'school'. Their work has been unified by interest in the problems, and not by any theoretical presuppositions. Certainly their method has worked well in this survey of the wide field indicated by the title of their book.

The sub-title of the book is significant. The writers have made no attempt to add to fact or theory; they have confined themselves to a survey of the problems which have been already raised and others which they judge to be important for the future, together with a critical review of the methods available for their solution. The book has a scope considerably wider than that commonly ascribed to social psychology, and it may, perhaps, be best described as a guide to the application of psychology to the study of society.

We may say at once that the book is indispensable to all students in this field, and that it is a necessary prolegomenon to advanced specialized study. Considered thus as a tool, its adequacy must be commended. The wealth of material is supplemented by excellent bibliographies at the end of each chapter, and there is a very good index. The need for brevity has led to an austere style, which maintains a high level of clarity throughout. All in all, the volume fully justifies the great labour which has been spent upon it. An adequate review could only be written after the book had been fully tested in use for a long time. Its potential value cannot be fully judged by detached reading. It is primarily as an instrument for research that it must be judged.

Taken as a book among books, it must be accepted with the defects of its qualities. The authors of its nineteen chapters have in all cases attempted to achieve a very comprehensive survey of the material assigned to them, and in consequence their essays are so successfully compressed that the literary remains of Aristotle seem verbose by comparison. So much compression, with so much detail, makes the book hard reading, and to some extent defeats the writers' purpose. Brief mention of names, each in a single sentence stating the nature of the contribution made by the writer quoted, is insufficient to enable the reader to judge, select, or even to understand fully. A student already at home with the topics discussed will not require the information offered, the beginner will find it inadequate. No one can make the fundamental ideas of statistical methods more clear than Dr Thouless (chap. VI, "Scientific Method and the Use of Statistics") and Dr Blackburn's account of intelligence tests is admirable, yet it may be thought that the research worker who set out with no greater knowledge than he could obtain from them would be dangerous. They would reply that such was not their intention, that they had informed the reader of the existence and value of the methods discussed, and that they had offered very full guidance for further reading. It is in fact the case that the book provides remedies for the limitations we have noted. To the slovenly reader it will be quite useless, but to the real student it will be of constant value. Probably no one reader will need all the chapters equally, but all who propose to carry on work relating to society will profit by some, and read their special sections of the volume to pieces. The first reading will warn the beginner that much has been done and that still more remains to do. He may be repelled by the difficulties which are made clear, and choose the path of facile speculation, but those who matter will be stimulated by the example of honest hard work before them.

To select particular chapters for comment is unfair, since the choice would depend on one's own interests. But since the first two chapters cover the common field of interest it may be proper to remark that the one by Prof. Pear ("Some Problems and Topics of Contemporary Social Psychology") would be better for some reorganization. The compression becomes more obvious in the later paragraphs, as is true of several other chapters. Prof. Bartlett ("Suggestions for Research in Social Psychology") ends with a list of topics ripe for investigation, but the earlier part is more argumentative, and will appear to some as surprising. Having shown that much useful work in social psychology may be done in the laboratory, he goes on to argue that even the processes involved in the psycho-physical methods may be brought under tribute. We should have been even better pleased if he had pressed his argument beyond the study of individual differences. Those who hold that the boundary between social and individual psychology is drawn by considerations of practical convenience rather than by essential differences, must hold that every experimental situation is of necessity a social situation, and, as such, capable of providing data for social psychology. Experimental psychology has undoubtedly been handicapped by the conflicting results which are so often reached in different institutions. Has it been sufficiently realized that though one may completely standardize a technique, there can be no standardization of the persons taking part? If there is integration of mental processes then the social grouping created by the experiment must affect the results. So Prof. Bartlett's views may be profitably extended to the point at which all that we have so far known as experimental psychology is made to provide material for social studies.

But commendation of particular chapters implies no adverse criticism of the others (and a word of special praise is due to the masterly essay on the "Problems and Methods of Sociology" with which Prof. Ginsberg concludes the book). Though the wood is often hidden by the wealth of trees no one would wish there had been less detail. They may wish there had been two volumes. And having read and used this work they will hope that the group which has sponsored it will issue further studies of equal importance and authority.

A. W. Wolters

Principles of Economic Sociology. As illustrated from the Bantu peoples. By D. M. Goodfellow. London: Routledge. 1939. Pp. xx + 289.

In recent years the economy of primitive societies has greatly attracted the attention of anthropologists. The present book, written by an economist with sound anthropological training, represents a somewhat different approach in that it attempts not only to examine the economic system of certain primitive African communities but also to test the conceptual framework of modern economics on this new material. It is a tenet of modern economic theory that, in economics, we are dealing not with a specific, well-defined field of social activity, but rather with relations occurring between social activities of any type. Economic relations thus represent an aspect of social life at large; they crystallize in the comprehensive system of evaluation that embraces all social acts, and becomes visible in the 'alternative choice' of individuals fitting existing means to felt wants. Evidently, this final balance between means and felt wants—the 'economic decision'—is the resultant between various, possibly conflicting, motives and incentives, and is thus essentially a psychological reality. Economists do not deny this psychological implication of their doctrine. But they are content with assuming or accepting the existence of these psychological factors behind the evaluations with which they are primarily dealing; they can leave the psychological processes themselves outside their framework of reference, being concerned, not with their nature and causes, but only with their final results, that is, in modern exchange-economy, with price-systems, supply-and-demand relations, and similar symptoms of the effectiveness of wants and incentives. In primitive societies, however, where the superstructure of formalized economic relations is reduced to much simpler dimensions, and where, moreover, the scale of values is unfamiliar, economic analysis is more directly brought up against the underlying psychological factors. In this lies the great interest of such economic studies of primitive societies to the psychologist, specially the social psychologist. In Dr Goodfellow's book this interest is amply sustained. His analysis, while fully vindicating the applicability of modern economic theory to simpler societies (a point sometimes questioned by anthropologists), attempts to unravel the network of motives and incentives behind the varying and, from our viewpoint, often unusual economic decisions typical of African societies—from the familiar profit-motive or values determined by religious convictions to the more vaguely defined incentive of conforming to accepted usages, an evaluation which is sanctioned more or less severely by public disfavour or 'loss of caste'. An approach of this kind involves a far-reaching examination of the social system which is the carrier of these values. It is with reference to this extension of the economic analysis over the field of sociological and partly psychological background factors that Dr Goodfellow calls his study a study of economic sociology rather than of economics proper.

At the same time the author carefully distinguishes his subject-matter from motives and incentives of a more abstract and impersonal kind—the 'needs' and 'social demands' of many students of society. He makes it clear that 'needs', biological or psychological, represent essentially methodological assumptions, and remain outside of the field of social or economic investigation, entering it only when transformed into concrete, and concretely observable, *wants*. Yet side by side with these concrete wants certain other wants of a more super-individual nature seem to exist in society. Dr Goodfellow's discussion of these 'collective' wants, which some economists and sociologists have accepted as a specific category, is of special interest to the psychologist. Dr Goodfellow argues the fallacy of the distinction between collective and 'individual' wants on the grounds that all wants are in some degree 'collective', as individuals invariably act as members of groups and under the dictate of values accepted in these groups. However, the same individual acts as member of different groups, of varying permanence and inclusiveness, like house-group, family, clan, village, tribe; it is thus possible that the values peculiar to these different social units differ from, or even contradict, each other. The problem is essentially the same as that with which the psychologist is dealing when examining motivation in a social setting. For there, too, we are often faced with an apparent divergence between individual and collective values and interests which, on closer analysis, reveals itself as a divergence between group-determined values and interests converging, owing to the very structure of society, upon the same individual.

S. F. Nadel

Introduction to Psychology. Edited by E. G. Boring, H. S. Langfeld, and H. P. Weld. New York: John Wiley and Sons (London: Chapman and Hall). 1939. Pp. xxii + 652. $3.00.

In the form of its first edition, as *Psychology, A Factual Textbook*, this treatise is already well known and appreciated in this country. It has been utilized with success as the basis for several University courses, and has won the good will of a number of teachers. The present edition has been very considerably expanded and altered, and in particular the arrangement of material is entirely different. The editors say that "since the student comes to psychology with some knowledge of the functioning of the organism in a social environment, it is best in an elementary course to begin with this knowledge—the problems of conduct, motivation and personality—and to proceed thence analytically to the simpler functions". This change certainly reflects the modern tendency to stress strongly the importance of social factors in the determination of behaviour, but whether it represents a better teaching order than the older one remains to be seen. The book still remains well-informed, not tied up with any particular school, and very interesting to read. Some teachers may regret its expanding length and hope that this will now go no further. But undoubtedly as a reliable and excellent text-book for the rather good type of student, it would be difficult to do better than this, and the editors and collaborators are once again to be congratulated on their achievement.

Lecture Demonstrations for General Psychology. By NORMA V. SCHIEDEMANN. University of Chicago Press. (Cambridge University Press.) 1939. Pp. x + 241. 12*s*. 6*d*.

The plan of this book is to select various fairly easy demonstrations which can be carried out without much apparatus, to describe them very briefly and to indicate generally the kinds of results which may be expected. Sixty demonstrations are included. They vary an immense amount among themselves in regard to their reliability and to their psychological significance, and it is rather a pity that no attempt has been made to evaluate these variations, so that a class-leader who has not himself acquired an expert knowledge of psychological experiment may sometimes be led astray. Still the book should serve a useful purpose, especially where, as with many extra-mural classes, it is not easy to obtain much apparatus, while still some fidelity to experimental method is desired. American influences are predominant in the selection of experiments for description, and very nearly all references are to American authors. No small part of the value of the book is that it will suggest to the ingenious class-leader many further demonstrations not here described but developing out of the ones selected.

Personality Changes after Operations on the Frontal Lobes. A Clinical Study of 32 *Cases.* By G. RYLANDER. Copenhagen: Einar Marksgaard (Oxford University Press). 1939. Pp. 327. 15*s*. net.

This book constitutes the largest single contribution to the literature of the Frontal Lobes in their relation to mental function since the publication of Brickner's intensive study of one case in 1936. Dr Rylander's investigation, however, presents another mode of attack. Choosing thirty-two cases, each representing a partial lobectomy carried out from 6 months to 7 years previously (mean 2½ years) in Prof. Olivecrona's clinic, Rylander visited them in their homes, made enquiries about their lives and mental status generally, and carried out a clinical examination and a number of psychological tests. A notable feature of the work is the repetition of these tests upon a control collection of thirty-two normal individuals, each of whom was chosen to resemble one of the operated cases as closely as possible in sex, age, occupation, education and general intelligence. The test-battery comprised a selection of tests of attention, of memory both rote and 'logical', and of intellectual functions such as capacity to appreciate general significance, to react 'categorically', to comprehend distinctions between abstract ideas, and to understand the meaning of fables and proverbs. In addition I.Q.'s were determined with the Stanford Revision.

It is no disparagement, either of Dr Rylander's careful and valuable work, or of the use of psychological tests in the study of the mental changes resulting from brain injury, to say that no novel or striking conclusion emerges from the whole investigation, and that the test results themselves afford but small direct contribution to the generalizations he is able to make. The value of the tests lies less in the quantitative assessment which they allow than in the fact that each provides a standard and repeatable situation to which the patient can be made to react. These samples of the patient's behaviour will afford a basis of comparison with future cases in many respects better than that deriving from casual observation of response in uncontrolled everyday circumstances. We may hope that with the accumulation of further case-material along the same lines new and interesting generalizations may emerge. The more clinical side of the study must be read in detail for its value to be appreciated. To the present reviewer's mind, one of the most interesting features is the apparent capriciousness of association between the various elements of reaction to lobectomy. In some cases, for instance, a general increase of activity is found, but this may be associated either with a decrease, or with apparent maintenance, of the capacity to direct this activity to adaptive ends. The reports also include much material for speculation, although little for conclusion, regarding the dependence of the whole reaction of the operated patient upon his previous personal characteristics. It is perhaps a pity that the author did not press his analysis of results further in this direction.

Most books printed in English by foreign presses may be counted upon to afford some typographical curiosities. A few gems are to be found here, notably the report that one patient was in the habit of taking *bromine* to calm herself.

Psycho-Analysis. By EDWARD GLOVER. London: John Bale Medical Publications, Ltd. 1939. Pp. 139. 12*s.* 6*d.* net.

This book ought to be certain of a wide welcome and a careful reading. It will be of value not only to medical practitioners and students, and to psychologists generally, but to a great many other readers as well. There was need for an authoritative and concise exposition of psycho-analysis, its theories and to some extent its practice, and the need is now well met. There is a brief, very sensible and moderate Introduction. Then comes Section I, which is a study of the theoretical foundations of psycho-analysis. Section II is concerned with the application of these theories to special problems of the more abnormal type. Section III gives a very short but very excellent statement of the actual methods of practice adopted by the psycho-analyst, in so far as these can reasonably be incorporated in a book of this scope and kind. Any instructed reader will recognize at once that to the making of this book an enormous amount of work must have gone, and Dr Glover is most warmly to be congratulated on an achievement which is both brilliant and timely.

Modern Psychotherapy. By NOEL HARRIS. London: John Bale Medical Publications, Ltd. 1939. Pp. 146. 7*s.* 6*d.* net.

Dr Harris's book is suitable for the general medical practitioner, for the medical student, and as an introduction to the subject for any intending psychotherapist. It will also be of interest to the ordinary student of psychology who wishes to know something about this field although he does not intend to practice in it. What is attempted is a quick and not complicated survey of the general realm of psychotherapy, an indication of the types of disorders best dealt with by specific methods, some practical guidance in the use of the methods, and the presentation of just enough illustrative material to keep the whole treatment on a fairly concrete level. This, as the author fully recognizes himself, is so wide-sweeping an aim that his treatment can hardly hope to satisfy any person who is particularly interested in any one set of theories or any special cult. Dr Harris deals with analytical methods, with suggestion and persuasion, with re-education, with occupational therapy and with child guidance. The writing, though not particularly good or distinguished, is certainly clear and untechnical as far as is possible. It seems a pity that the bibliography should indicate only two books dealing with problems of normal reaction. The two selected are good, but they represent only one point of view, and they leave out of account a large number of topics which are wholly relevant to the purposes of psychotherapy.

Guide to Mental Health. By H. D. JENNINGS WHITE. London: the C.W. Daniel Co., Ltd. 1939. Pp. 298. 15*s.* net.

From the symptoms of mental disorder, which he has observed in the course of psycho-therapeutic practice, Dr Jennings White has deduced that men seek as a rule to attain one of six goals—duty (including obedience and self-sacrifice), power, love, humility, health and happiness. In so far as they are obsessed by the importance of achieving these, out of all harmony with biological necessity, and by intense fear of losing them, they will exhibit characteristic neurotic, psychotic or criminal tendencies. In the past the pursuit of these goals has been unwisely encouraged by certain forms of religion—duty and self-sacrifice by Christianity, power by Mohammedanism, happiness or bliss by Buddhism and mysticism. Dr Jennings White analyses the manner in which the harmful influence of these cults seems to have arisen, with a view to destroying its harmfulness, while retaining what is valuable in a sounder ethical, philosophical, scientific and educational system which he calls Eutrophy. This encourages the pursuit of the above goals in moderation, with acceptance of partial failure and without inordinate fear of loss.

Dr Jennings White's theory would be in itself both plausible and suggestive, were it put forward in less hyperbolic language, and with less insistence on the 'nothing but' fallacy. Thus almost all the evils described are due to 'nothing but' the influence of the wicked parent (who might sometimes have emerged from the pages of *Ruthless Rhymes for Heartless Homes*). No consideration is given to innate differences of motive, temperament, intelligence, etc. It is absurd, for instance, to imply that the experiences of Gautama Buddha were due simply to the fact that his father spoilt him in childhood.

If Dr Jennings White could describe his psycho-therapeutic theory and practice in clearer and soberer terms, with more illustrative case histories and less somewhat hypothetical biographical material, his work should afford a valuable contribution to the study of motivation.

The Clinical Treatment of the Problem Child. By CARL R. ROGERS. London: George Allen and Unwin, Ltd. 1939. Pp. xiii + 393. 15*s*. net.

This book forms an excellent compendium of information about the methods of diagnosis and treatment of maladjusted children in the U.S.A. It consists of three main parts, the first on diagnosis, the second on treatment through modification of the environment, and the third on direct treatment of the child himself. The second section is far the best, particularly in its most interesting description of the results of foster home and institutional placement, and of the characteristics of good foster homes and institutions. These are all of course American; a corresponding study of English institutions would be most valuable.

The first and third parts of the book are somewhat superficial, suffering from a common tendency towards behaviourism and a lack of consideration of basic tendencies and mechanisms. The book thus contains little of great importance to the psychologist. But the sociologist and the social worker (especially in mental hygiene) should find the second section in particular most useful and suggestive.

The Dilemma of Penal Reform. By HERMANN MANNHEIM. London: George Allen and Unwin, Ltd. 1939. Pp. 238. 7*s*. 6*d*.

Into this book an immense amount of information is crammed. Anybody who wants to get a brief but reliable indication of what all sorts of people, at all sorts of periods, have thought and practised about the treatment of criminals could hardly do better than begin here. Especially, there are some good summaries of recent Russian and German work which is not otherwise easily accessible. The general dilemma is, of course, how both to protect society and to assist the offender. Dr Mannheim is not concerned very much with moral considerations, which he rightly thinks may often be so treated that they obscure both theoretical and practical treatment, but he pursues his study of the main dilemma in the realms of economics, sociology and law. He is on the whole a little critical of psychological approaches to the study of criminology, holding them to be, in some not very clearly defined manner, inferior in objectivity to sociological methods, a view which is true only of some psychological approaches. The discussions throughout, rather heavily weighted with learning as they are, are good. The conclusions are sensible, and mild.

The Psychology of Common Sense: A Diagnosis of Modern Philistinism. By A. A. ROBACK. Cambridge, Massachusetts: Sci.-Art Publishers. 1939. Pp. 350. $3.00.

It is a little ungracious to gird at a title; nevertheless, if any author proclaims himself an expositor of "common sense", difficulties arise. Dr Roback is fully aware of these and in his preface he discusses the notion of common sense. Without arriving at anything very definite he prefers to regard this kind of sense as "the common residue which settles out of the various individual experiences". This suggests an experiment, not more difficult or more odd than many of those suggested in the book itself by Dr Roback. It would be interesting to take any of these essays and to present it to a random selection of any human population of sufficient numbers, and then to record the number of statements that would significantly arouse dissent. When, a little later, Dr Roback considers his sub-title, he remarks that "the philistine is the spearhead, if not the bulwark, of common sense". It seems quite possible that the experiment just suggested might show Dr Roback to be a singularly effective lancer, but a little less successful as a chemist distilling some 'common residue'. Be this as it may, the collection of essays which he here presents is certainly highly attractive to read, will start any adventurous psychologist on all sorts of trains of fertile pursuit, and will yield him amusement and pleasure as well as no small amount of genuine instruction. The essays are on many different subjects, and a good many of them are highly topical. No discussion is completely followed through, but that makes them all the more effective as stimulants of lively discussion.

The Psychology of Making Life Interesting. By WENDELL WHITE. London: Macmillan and Co. 1939. Pp. xv + 215. 11*s*. net.

Dr Wendell White writes on Dealing with People in Life Situations in General, on Preventing Unwholesome Behaviour Due to Tedium, and on Furthering Mental Health. In general the book has one idea: it is that what is required to make life interesting is variety. The author has brought together a large number of stories and aphorisms, good, bad and indifferent, and a larger number still of unexceptionable and familiar generalizations. Readers who like this kind of thing may like this instance of this kind of thing very much indeed.

Towards Relationship. By FLORIDA SCOTT-MAXWELL. London: John Lane, The Bodley Head. 1939. Pp. 253. 7*s*. 6*d*. net.

Books on popular psychology are difficult to write and, as a rule, singularly unconvincing to read because of their superficiality. But this book presents an honest and thoroughgoing attempt to understand and analyse the nature of the feelings and attitudes of English women in their relationships to society, and in particular to their husbands and children. Its main thesis is that because the position of women in society has so greatly altered in recent years, they are finding it hard to attain a satisfactory adjustment of these feeling relationships. This thesis is expressed with real psychological insight, in language which it is a pleasure to read, and without any technical psychological terminology or theory. In fact, the only difficulty it presents to the general reader is the difficulty inherent in its subject, one which is of singular importance nowadays, particularly to women.

The End of Fear. By DENIS SAURAT. London: Faber and Faber, Ltd. 1938. Pp. 85. 6*s*. net.

This is a poet's work, translated from the French with sympathy and understanding and with a delicate regard for language. No psychologist is likely to read it without being moved and intensely interested, but he is also likely to be the first to agree that it is not to be adequately appraised by psychology alone. The book tells, in a series of dramatic and condensed conversations, how the author returns to his home after a long absence, and talks with his mother after the death of his father. The talks are a series of recollections of incidents which perhaps appeared to be slight and of temporary interest when they occurred but which now seem to hold deep significance. The mother and the son have known fear, anger and abiding disquiet. Now together they find what appears as the end of fear. How they find it must be left to the reader, who, however, should neglect neither the Preface by Philip Mairet nor the Commentary by Neil Montgomery.

An Autobiography. By R. G. COLLINGWOOD. Oxford University Press. 1939. Pp. 139. 7*s*. 6*d*. net.

This is certainly a book to read. Prof. Collingwood says "The autobiography of a man whose business is thinking should be the story of his thought. I have written this book to tell what I think worth telling about the story of mine." So, there is not very much in it in the way of personal events and anecdotes, though what there is, is full of interest. But the story of the thinking, its development, its clearly and emphatically stated issues, its relation to international movements, is beautifully told, without a word too much and with an honesty which will command admiration even where it induces dissent. Psychologists, indeed, will find a few emphatic and disapproving statements here (especially on pages 93–5) which will seem to them to be unnecessarily exaggerated and misleading. It appears to be indicated that psychology is all right so long as it sticks to a study of sensation, appetite and the emotions connected with them, or as long as it confines itself to psychotherapy, but that outside of these fields it is merely silly. Here perhaps Prof. Collingwood is charging all psychologists with the errors of some, and even pushing the claims that he dislikes further than they were meant to go. But it does not matter. The book is not for agreement or disagreement about detail, it is for appreciation of the whole, and especially for that part which has to do with the relations of the study of history and the study of philosophy. The final parts, dealing especially with recent events, the growth of Fascism, the Spanish struggle, the relation of both to English political ideas and practice, is very honest and outspoken and of tremendous interest. Yet now it seems as if the book has stopped before it ought to have done. There may be a sequel, for perhaps many of the ideas that here seem to be presented as near dead may come alive again and move the world.

Knowledge and Character. By MAXWELL GARNETT. Cambridge University Press. 1939. Pp. 358. 18*s*. net.

Like Dr Garnett's earlier book, ***Education and World Citizenship***, this work is concerned with the aim of education. The author is able to show that some of the suggestions he made earlier have been since made in the Hadow and Spens reports. The chapter on the analysis of ability is excellent; much of the rest of the book suffers from its author's lack of interest in experimental research in other fields of education. His theory of ***neurograms*** is simply and clearly explained. Our judgement of its value will depend on our opinion of the explanatory value to psychology of speculative physiology.

Apparitions and Haunted Houses. By ERNEST BENNET. London: Faber and Faber, Ltd. 1939. Pp. xix + 396. 12*s*. 6*d*. net.

The Very Rev. the Dean of St Paul's, in his appreciative Introduction to this book, says: "The study of Apparitions and Haunted Houses which Sir Ernest Bennet has given us in this book is not designed to satisfy that craving for the marvellous which is, I suppose, innate in most of us, but rather to awaken scientific curiosity." Let nobody suppose, however, that the book is dull or uninteresting, or that anybody who reads it, in moments when he is seized with a not wholly scientific curiosity, is likely to be bored. Most of it, after a brief preface and a reprint of a Broadcast made by the author in 1934, consists in case histories with brief comments. There are 101 instances of "Recognized or Identified Apparitions, of Unidentified or Doubtful Apparitions, of Apparitions of the Living, of Apparitions of Animals", and three cases of Apparitions of Inanimate Objects.

The Summary and Conclusions are briefly and very clearly expressed. Sir Ernest Bennett is himself inclined to accept the view that the evidence must be interpreted as supporting the hypothesis that all apparitions are due to telepathic influence. He faces quite fairly the problems that this view raises with regard to apparitions of dead persons, and of animals alive or dead. But whether the evidence is accepted in this sense or not, most psychologists who try it will enjoy the reading of the book, and perhaps can be left to draw their own conclusions.

Experiments in Telepathy. By RENÉ WARCOLLIER. London: George Allen and Unwin, Ltd. 1939. Pp. viii + 250. *7s. 6d.* net.

M. Warcollier is a French chemical engineer, who for twenty-five years has been attempting experiments on telepathy, most of them connected with the transmission, without ordinary means of communication, of pictures and drawings, or of whatever mental processes seem to be involved in the reproduction of pictures and drawings. In this book he gives a full account of his work, together with many illustrations. There is no attempt at elaborate statistical analysis. This is mainly because the author is convinced that telepathy is a product of two factors, one psychological and the other some unknown psychical influence, and he devotes a great part of his discussion to an attempt to work out the psychological factor in detail in a qualitative manner. Whatever one may think of the validity of his interpretation of some of his case material, there is no doubt that the attempt to disentangle some of the alleged psychological processes at work is of considerable general interest. Broadly the view is that what may be transmitted are images, ideas, emotions, but not meanings nor 'thoughts'. Actually, the psychological equipment of the author is not tremendously impressive, and his views require much and careful critical analysis. Still the book is an honest and persistent attempt to introduce some orderly notions into a disorderly field, and to that extent it perhaps merits the appreciative Foreword written by Dr Gardner Murphy.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY
1939

GENERAL MEETINGS

March 30–April 3, 1939. Extended General Meeting at Reading

March 30. 1st Session:

Business Session, followed by a Piano Recital by Martha Vidor.

March 31. 2nd Session:

"An Artist under the Sea." By Robert Gibbings.

"Reason in Painting." By J. Anthony Betts.

"Paintings from a Child Guidance Clinic." By C. L. C. Burns.

3rd Session:

Demonstration of apparatus for investigating the influence of Adaptation on Reading-time, including an improved design of Voice-key. By W. Douglas Seymour and D. F. Vincent.

4th Session:

"Pleasure and Freudian Theory." By Margaret Lowenfeld.

5th Session:

Discussion on "Psychologists and National Service", opened by the President.

April 1. 6th Session:

Symposium on "Factor Analysis."

Papers by Godfrey H. Thomson, C. Spearman (read by Maxwell Garnett), C. Burt, Wm. Stephenson.

7th Session:

"Mental Type and Body Measurement." By Bernard Armitage.

8th Session:

"Influence of Short Rest-pauses on Fluctuations of Attention." By F. W. Warburton.

"General and Specific Factors in Aesthetic Judgments." By H. J. Eysenck.

9th Session:

"Rhythm in Voluntary Action." By A. Michotte.

April 2. 10th Session:

"Mass Observation and its Methods." By Celia M. Fremlin.

"A Study of Parental Dominance and Submission." By Percival M. Symonds.

11th Session:

"Examining Musical Ability." By Martha Vidor. (Illustrated at the piano.)

12th Session:

Open Session for Discussion.

July 8, 1939.	General Meeting at University College, London. "An Experimental Investigation into the Development of Self-control in Children." By S. H. Cracknell. "Fluctuations of Meaning in Binocular Rivalry." By J. C. A. Corea. "The Repetition of Dr J. B. Rhine's work in Extra-sensory Perception." By S. G. Soal. "An Experimental Investigation of the 'Ultra-perceptive' Faculty." By John Hettinger.
December 9, 1939.	Annual General Meeting. "Experiments on the 'Paranormal Cognition' of Drawings." By Whately Carington.

SECTIONAL MEETINGS

Medical Section

January 25, 1939.	Address from the Chair: "The Psychotherapy of the Psychoses." By Denis Carroll.
February 22, 1939.	Joint Meeting with the Royal Medico-Psychological Association. Papers on "Psychiatric Nursing". By R. D. Gillespie and Noel Harris (for the Medical Section); and A. A. W. Petrie and J. Ivison Russell (for the Royal Medico-Psychological Association).
March 22, 1939.	"Changes of Character during Psychological Treatment as shown in Changes of Handwriting." By H. J. Jacoby.
April 26, 1939.	"The Dual Aspect of Time in Psychotherapy." By J. C. Mackwood.
May 24, 1939.	"The Subjective Experience of Illness." By E. Guttmann and W. Mayer Gross.
June 28, 1939.	Open Discussion on "The Recognition and Training of Non-medical Psychotherapists." Principal Speakers: Aubrey Lewis, J. Leycester King, R. D. Gillespie, Mark Palmer, Ernest Jones, H. G. Baynes, J. R. Rees, Hazel Chodak Gregory, Denis Carroll.

Education Section

January 4, 1939.	"Anxiety and Fear in the Child." By J. A. Hadfield and Grace Calver.
February 13, 1939.	"Group Play Therapy. Theory—Practice—and Some Considerations regarding Training." By Margaret Lowenfeld.
March 13, 1939.	"The Innumerable Instincts of Man." By C. A. Claremont.
April 28, 1939.	"Psychology and Musical Education." By Martha Vidor.

May 20, 1939.	Discussion on "Education and Propaganda". Opened by Morris Ginsberg and R. H. Thouless.

Industrial Section

February 8, 1939.	"Intelligence and Birth Rate." By Lionel S. Penrose.
March 10, 1939.	Discussion on "The Functions of the Industrial Psychologist and of the Personnel Manager." Speakers: A. V. S. Lochhead, E. B. Sharp, E. R. Brady, W. Raphael.
April 27, 1939.	"The Birmingham Experiment and Entry to Junior Technical Schools." By E. P. Hunt.
May 4, 1939.	"'The Destroyers' or the Importance of Psychological Study for the Administrator." By Elton Mayo.
June 8, 1939.	"Occupational and Economic Factors of Mortality." By Major Greenwood.
December 7, 1939.	"Problems of Space and Time Orientation arising in Peace and in War." By G. H. Miles.

BRANCH MEETINGS

Session 1938–39

Scottish Branch

November 5, 1938.	(Edinburgh). "The Integration of the Personality." By Karl Abenheimer. "Occupational Therapy in the Treatment of Convalescent and Neurotic Patients." By Winifred Rushforth.
December 3, 1938.	(Glasgow). "The Basic Intelligence of Scottish Children." By M. MacMeeken. "The Social Psychology and some aspects of International Association Football." By R. W. Pickford.
February 4, 1939.	(Edinburgh). "Intelligence Testing of Nursery School Children." By M. Drummond. "High Scorers in the Scottish Research Council's 1932 Mental Survey." By Ben Morris.
March 4, 1939.	(Glasgow.) "A New Approach to the Testing of Musical Ability." By B. Semeonoff. "Some Experiments in the Psychology of Music." By J. Meiklejohn.

MIDLAND BRANCH

October 6, 1938.	Debate on "Of what use are psychological methods in school to-day?" Opened by: L. Bradley and C. Rogers.
November 6, 1938.	"Recognition of Mental Strain in Industry." By H. Collier.
January 2, 1939.	"Psychological Experiments at Coventry Technical College." By H. Rochester.
February 22, 1939.	"The Innumerable Instincts of Man." By C. A. Claremont.

NORTHERN BRANCH

June 24, 1939.	(Liverpool.) "The Emotional Concomitants of Left-handedness." By W. E. Moore. Progress Reports on Research Work at Liverpool: (*a*) "Vocational Information for Secondary School Girls of Various Ages." By Joan Lingwood. (*b*) "Individual Differences affecting the Response of Stammerers to One Form of Treatment." By E. Pike. "Occupational and Vocational Guidance." By K. H. R. Edwards."

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